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THE PRACTITIONER:

III

A JOURNAL

OF

THERAPEUTICS AND PUBLIC HEALTH.

EDITED BY

FRANCIS E. ANSTIE, M.D. F.R.C.P.

*Physician to Westminster Hospital ;  
Lecturer on Medicine in Westminster Hospital School ; and  
Physician to the Belgrave Hospital for Children.*

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# THE PRACTITIONER.

JANUARY, 1874.

## Original Communications.

EMPHYEMA OF LONG STANDING.—REMOVAL IN THE COURSE OF TWO DAYS, BY PARACENTESIS, OF NINE QUARTS OF PURULENT FLUID FROM THE THORAX.—DEATH FROM PURULENT INFECTION.

BY JOHN W. OGLE, M.D.

ALEXANDER F., aged 33, a clerk, six feet in height, was admitted into St. George's Hospital June 12th. He was born of healthy parents, and had always been hale and strong until he was 25 years old, when, after much exposure, he caught "cold," to which he ascribed his illness. The "cold" was followed by hæmoptysis, which lasted several weeks. He subsequently became much troubled by palpitation of the heart, dyspnœa, and constant cough, which greatly reduced him; and for *three* years he had been quite unable to lie on the right side; and during that time his legs had been anasarcaous. For eight months he had had insufficient food.

SYMPTOMS ON ADMISSION.—He was very emaciated, and his cheeks, lips, and nose were very livid. His breathing was short and rapid (being 36 per minute), and laboured: and the respiratory movements were almost entirely confined to the right side of the chest. He lay turned over to the left side. The chest was remarkably "barrel-shaped," and the ribs on the left side

bulged out much more than those on the right, the intercostal spaces being prominent. The left half of the chest measured three inches more than the right half (the proportion being 17 to 20 inches). On percussion and auscultation it was found that whilst the respiration was puerile over the entire right side (excepting at the lower portion posteriorly), the whole of the left side was quite dull on percussion, and entirely devoid of respiratory murmur, the vocal resonance being absent. The heart was pushed over to the right side, so that its impulse was felt the strongest about one inch to the *right* of the right nipple: it was beating at the rate of 225 per minute.

The urine was scanty, loaded with lithates, and very albuminous.

The patient was supported by wine and nourishments, and on the day following admission two quarts of purulent but fœtorless fluid were drawn off by the aspirator from the cavity of the left pleura, the needle being introduced between the sixth and seventh ribs. This gave the patient immense relief, for whereas prior to the operation the respirations were 45 per minute, in twenty minutes afterwards they were only 30. Before the operation the temperature was found to be  $97.6^{\circ}$  and very shortly afterwards it was  $99.6^{\circ}$ . The palpitation of the heart was also greatly ameliorated by the operation. Much fluid was still left in the pleural cavity. On the 14th the pulse was 220 per minute. On the 15th the temperature was  $98.2^{\circ}$  at 9 A.M., and  $98.6^{\circ}$  in the evening. On the 16th, note was taken that the patient could sleep and breathe with comparative ease, the cyanosis having almost entirely departed. On that day the temperature was  $98.6^{\circ}$  at 9 A.M., and  $99.6^{\circ}$  at 9 P.M.

On the following day (the 17th) there was increased dyspnoea and palpitation, and ether and digitalis were given with great relief: but still the patient was unable to lie on the right side. The urine was, as before, very albuminous. The temperature was  $99.8^{\circ}$  at 9 A.M.

From the above date to the 27th, frequent examination showed the existence of the following state of things. The whole of the left side of the chest was dull on percussion. Vocal resonance was deficient, and vocal fremitus quite absent

No expansion of lung could be detected. The apex-beat of the heart was felt chiefly beneath the ensiform cartilage, but a second kind of beat was also recognised directly below the right nipple. The dyspnœa was not very troublesome, but attacks of palpitation were so; and during their occurrence the pulse became extremely irregular, so that it could not be counted. At other times the pulse was, generally, about 108. The urine remained scanty, but had *ceased to be albuminous*. Fluid in the abdominal cavity was detected. As the symptoms suddenly became more urgent, paracentesis was again performed by the house surgeon, Mr. Morgan (an ordinary trocar being used), an incision being made between the fifth and sixth ribs; and slowly and by degrees no less than seven quarts of purulent fluid (of the same nature as that removed by the aspirator) were drawn off. Mr. Morgan then passed through the incision a long bent probe, made it point between the ribs at the bottom of the thorax (the ninth and tenth), cut down upon it, and passed through the lower opening with a drainage-tube attached. Owing to this procedure the dyspnœa was greatly relieved, and the palpitations ceased: the anasarca in the legs also ceased. Subsequently, the apex-beat of the heart was felt about one inch to the left of the right nipple. Owing to the admission of air, the left side of the chest became very resonant, and the subcutaneous tissues around the wound were emphysematous. The patient much improved; his appetite was good, and he took meat and other nourishment, and four ounces of brandy daily, also bark and mineral acid.

On the 1st of July he complained of losing strength, and of troublesome perspiration, and of headache. The purulent fluid continued to drain out from the canula, which on the 5th was for some surgical reason removed. On the 8th, the following note was taken by Mr. Spitta, acting as house physician:—"Much improved. The heart is now situated with its base corresponding to the cartilage of the fourth rib on the right side, whilst its apex may be seen pulsating just beneath the ensiform cartilage. Its action is quite regular. Tubular breathing may be heard at the upper part of the back of the chest. *Ægophony* cannot be detected." On the 9th there was great sweating and some shivering. On the 14th the patient suddenly became worse, and

died with all the symptoms of purulent infection, or blood-poisoning. In addition to the remedies and stimulants above mentioned, the patient from time to time took diuretics and sedatives, and occasional aperients.

POST-MORTEM EXAMINATION.—The left pleural cavity was found distended equally with air and purulent fluid, the surface being coated by recently formed fibrine, and the pleural membrane very thickened. An old and firm band crossed the upper part of the cavity. The left lung was completely devoid of air, and was firmly compressed against the spine at the upper part of the thorax. It contained numerous small excavations filled with inspissated pus and caseous material, and surrounded by dense fibrous tissue. On the right side the pleural sac was natural. The right lung was emphysematous and congested, containing small nodules of cretaceous deposit. The heart, which weighed twelve ounces, was much displaced towards the right. The kidneys weighed sixteen ounces, but their texture appeared natural. The other organs were natural.

REMARKS.—In the above case we possess, as regards the character both of general and physical symptoms, a good typical instance of empyema with consequent pressure on the lung, dislocation of the heart, and congestion of the kidneys. That the affection was of a phthisical origin may be inferred from the hæmoptysis which attended its early stage, and from the excavations in the lung containing caseous matter, &c., found after death.

The case presents several points of clinical and practical interest. In the first place, the lengthened period of duration of the pleurisy is worthy of note, as it would appear that it had at least existed as much as three years; and it was remarkable that during that time no positive help in the way of withdrawing the fluid in the chest had been suggested (a procedure which must for long have been requisite as well as feasible). Again, the good results of the removal of the fluid were strikingly and speedily manifested: and it was interesting though not surprising to find that the temperature of the body, which previous to the operation had been below the natural standard, quickly rose after the operation, two degrees. This comparatively low temperature before the operation was, no doubt, owing to the deficient



arterialisation of the blood and consequent failure of heat-producing processes, which resulted from the encroachment on the pulmonary functions that existed before the operation.

Notice should also be directed to the subsidence of anasarca and of dyspnoea and palpitation of the heart, as well as to the removal of the albuminous condition of the urine which ensued upon the relief to the various organs afforded by the paracentesis of the chest.

But especially it should be noticed as an interesting fact, that *within the space of two days no less than nine quarts* were evacuated from the chest; and it is indeed a matter of astonishment that at one operation seven quarts could be evacuated from one side of the thorax. In addition to this quantity, the pleural cavity (which practically was converted into an immense abscess) continued for some days to pour out purulent fluid through the drainage-tube.

The case, in addition to other things, shows that neither danger nor inconvenience need necessarily follow the removal at one time of large quantities of fluid from the chest; inasmuch as not only were the symptoms at the time ameliorated, but the patient continued to improve for many days after the operations, until, in fact, the symptoms of blood-poisoning, which caused his death, came on.

## THE THERAPEUTIC EMPLOYMENT OF BROMIDE OF POTASSIUM.

BY PROFESSOR BINZ, OF BONN.

THE introduction of bromide of potassium into therapeutics is due to a French physician, Dr. Pourché, an inhabitant of the same town in which bromine had been discovered, ten years earlier, by Balard. If only the half of what is vaunted about it at the present time be true, there is much reason to regret that its popularisation has been so long delayed. A salt so easy of application, and so free from danger, well deserved to have earlier become the panacea for a crowd of neuroses.

There are certain experiments, dating from the most recent period, which give me the opportunity to cast a glance upon the modern bromide of potash treatment. From a much earlier date—that of 1838—we possess a series of researches upon men and animals;<sup>1</sup> and (from a contemporary standpoint as to method) some of these researches deserve every acknowledgment. From this time a long-persisting ebb set in, until the almost universal acceptance of KBr within the last ten years had made a new research upon animals indispensable. I believe that this was first done by myself in Germany. My own labours on this subject give me a right to criticise the foreign researches.

The results obtained by me with puppies and rabbits,<sup>2</sup> in contrast to the previous therapeutical assertions, were but little reassuring. Chloride of potassium operated quite as

<sup>1</sup> See Hage, "Die seit 1830 eingeführten Arzneistoffe." Leipzig, 1861.

<sup>2</sup> Sitzungsber. der Nieder-Rhein. Ges. f. Nat. u. Heilk. vom März, 1867.



depressingly as the bromide; but bromide of sodium exhibited, even when the animals were, so to say, impregnated with it, an action equally wanting in any correspondence with the medical statements concerning its action on the heart and nerves with that of chloride of sodium. It appeared to me, on the whole, that as regards the action of KBr on the human organism, the properties of potash described by Grandeaun and Claude Bernard were to be considered in the first rank.

At the same time Eulenburg and Guttman were also occupying themselves with researches on KBr.<sup>1</sup> As they very quickly published the results of their researches,<sup>2</sup> the communication of my own particular research was delayed. They were the chief cause of the repetition of this research and of others upon the potash salts; since the above-named inquirers also came to the conclusion, from experiments on frogs and rabbits, that KBr, Na Br, and  $\text{NH}_4\text{Br}$  behaved in the same way with the other salts of these alkalies, and not in any specific manner.

Up to and in the year 1868 there appeared a mass of clinical records, the majority reckoning KBr as a nervine, only some few giving a negative result. W. Sander also preceded experimentally, and indeed on men.<sup>3</sup> In his short but clear exposition "Upon the therapeutic employment of KBr and KCl," he comes to the following result:—"KBr is only effective when given in large doses. If we give, at first, 78 grains daily (in  $3\frac{1}{2}$  ounces of water only), and gradually increase the quantity to half as much again, there occurs in many, but far from universally, an improvement in so far as that the number of the (epileptic) attacks diminishes, and the individual fits abate of their intensity and duration. In some cases, in which the regular and typical occurrence of the attacks had been watched for a long time before the bromide was given, the fits remained absent, under the remedy, for several weeks or even months. The employment of chloride of potassium gave the same results in epilepsy as the bromide of potassium, and I have not, so far, seen any case in which the chloride had failed and the subsequent use of bromide succeeded. The chloride, moreover, possesses the

<sup>1</sup> Centralblatt, f. d. Med. Wissenschaften. 1867, 18 Mai.

<sup>2</sup> Virchow's Archiv. 41, 91.

<sup>3</sup> Centralblatt, 1868, 317.

following advantages:—1. It has no injurious extra-effects (such as the acne-like eruption, &c., produced by the bromide); 2. Smaller doses of it are sufficient, since it contains more potassium than the bromide; 3. It is considerably pleasanter.”

Sander's clinical experience thus harmonised completely with the above related experiments on animals, yet it was but little heeded.

Lewizky's<sup>1</sup> work, “Upon the action of bromide of potassium upon the nervous system,” consists of experiments on frogs, and of clinical observations. The first part proves the same facts which all who had made researches on the potash salts had maintained. From the second I may adduce, as important for us on the present question, the fact that in three cases of insomnia—after dysmenorrhœa, in hemiplegia, and in amenorrhœa—quiet sleep was obtained by bromide of sodium in doses of 54 to 82 grains. That the bromide of potassium was also effective is left to be presumed. In the experiments on frogs it was found that KBr contracted the vessels (p. 192). This cannot, however, by any means be assumed to be proved; for the slower flow of blood from the stumps of amputated toes of a frog which has been “fully poisoned” with KBr may well be referred to the unquestionable poisoning of the heart and a consequent lowering of arterial pressure; and Lewizky's direct observations on the vessels of different parts gave results which were discordant with each other. Purser,<sup>2</sup> in the same circumstances, could not verify any alteration in the vessels. Everything is wanting, in this research, for the “rational foundation of the hypnotic action of KBr” on an artificially-produced anæmia of the brain. Lewizky says, also, without giving particulars, that he has observed the hypnotic action of KBr. on rabbits, dogs, and healthy men. Nothing is said about bromide of sodium.

The researches of G. J. Schouten,<sup>3</sup> of Leyden, upon the physiological action of KBr on rabbits and dogs give substantially a confirmation of what was known before. Personally, however, I must remark that in his literary introduction he makes me

<sup>1</sup> Virchow's Archiv. 15, 183.

<sup>2</sup> *Dublin Medical Journal*, 1869, p. 321.

<sup>3</sup> Arch. d. Heilkunde, 1871, p. 97.

say the exact contrary of what will be found printed in the particular passage of mine which he quotes. The mistake is so bad that it must have proceeded from a confusion of names. It would seem that I should express myself, further, against the conclusions of two French experimenters (Martin-Damourette and Pelvet). According to Schouten's own quotation, however, their works<sup>1</sup> are of distinctly later date than my communication to the Nieder-Rhein-Gesellschaft. I said, and could say, not one syllable about them.

Schroff, jun.,<sup>2</sup> like Lewizky in Kasan, Purser in Dublin, and Saison in Paris, found in KBr an antidote to strychnia poisoning in animals. This is no longer a surprise after all that had been already done on this subject by Guttman<sup>3</sup> and Podcopaew.<sup>4</sup> Everywhere it is the potassium, and the potassium alone, which encounters us with striking phenomena of depression of nerves and muscles. Meihuizen<sup>5</sup> further makes out that the action of large doses of potassium-salts (and bromide among them) in restraining reflex action consists in their controlling or preventing that portion of the reflex act which takes place within the spinal cord. (Compare also Eulenburg, Guttman, and Lewizky.)

Very few, and scarcely needing a commentary, are the experimental results of Laborde.<sup>6</sup> He took large doses of KBr (156 to 234 grains), and expresses himself, at p. 554, on the effect in the following way:—

"One feels as if taken with a general stupefaction, which invincibly compels sleep. This sleep is rather a state of heavy somnolence; it is often suddenly interrupted, although there are, properly speaking, no dreams, or no dreams that take a definite shape; it is rather a nightmare, and an indefinite nightmare. However, the state of somnolence is indefinitely prolonged, and waking is accomplished with great difficulty: the will seems as if it were lost, and as if one could not regain possession of one's ideas and one's self-consciousness: one feels plunged in hebetude and stupor."

The pulse was very slow, small, and soft; the extremities were

<sup>1</sup> Bull. du Thérap. 1867, pp. 241, 289.

<sup>2</sup> Mittheilungen aus d. Pharm. Institut. in Wien.

<sup>3</sup> Berliner Klin. Wochensh. 1865, Nos. 34, 35, 36.

<sup>4</sup> Virchow's Arch. 33, 505.

<sup>5</sup> Pflüger's Arch. 7, 209.

<sup>6</sup> Gaz. Médicale, 1869, Nos. 42, 45, 49, 52.

cool, and, for some eighteen hours, paretic. Laborde therefore took smaller doses (94 grains). Within an hour he felt "a general sensation of well-being, and of calm which invited to sleep": the latter, however, was but half established, so to speak: and in this half-sleep there soon came on, especially if one were lying in one's bed, a more or less intense sexual excitement, according to the circumstances, an excitement which was habitually accompanied by erection and emission; this act, of which there is always perfect consciousness, almost always wakes one. . . . Finally, sleep is definitely established, but with more difficulty or less rapidity than when emission does not take place." Besides this there was strong desire to urinate, and bad dreams: and this interesting scene closes with pains in the limbs and muscles on waking. Thus KBr is also an aphrodisiac. What manysidedness!

If now one takes up a monograph<sup>1</sup> on the "physiological and therapeutic action of the bromide of potassium and the bromide of ammonium," of 178 pages, one expects that new light will come to us from it, respecting the extensive scope of its title. That is not the case, however, with the American treatise of Clarke and Amory. The former author experimented on himself: 40 to 50 grains of KBr sometimes produced sleep, and sometimes watching. The explanation seems to him simple. KBr contracts the vessels of the brain: again, sleep is nothing but brain-anæmia; the question therefore is, whether the bromide finds the contents of the cranium hyperæmic or already anæmic; the results will be correspondingly different. The experiments of Hammond serve as a support to this view; he has verified, by direct inspection, the bloodless condition of the brain (in a trephined animal) after the administration of KBr; this was caused by contraction of the vessels. Of the possible heart-paralysis from large doses, and the consequent diminution of the blood-pressure, nothing is hinted. Especially it seems as if all the older observations on the potassium-salts as such had no existence for the two authors, since they are never alluded to. Eulenburg and Guttman's researches on KBr are only known to them through a French sub-editor; Lewizky's research (cutting off the toes) is represented as taking place somewhere in Berlin,

<sup>1</sup> Bromide of Potassium, &c. Clarke and Amory. Boston, 1872.



(without any express citation of the names of the authors), and as a valuable support for the theory of vasomotor action. Bromide of ammonium and bromide of lithium act "very similarly," on animals, to KBr; although in Amory's researches, of which (more than twenty in number) a few are detailed, one animal appeared "sluggish" under the poisoning with KBr, whilst the one poisoned with  $\text{NH}_4\text{Br}$  passed into opisthotonos. This so completely and wholly different a picture of a potash and an ammonia poisoning is drawn with only the one reserve, that they were "nearly" alike in every respect. Bromide of ammonium must—so it is said at p. 104—properly be given together with KBr, if it is to operate; nevertheless, it is a "happy sedative" agent. Where KBr does not succeed, one can often give it in conjunction with bromide of lithium; while bromide of sodium is but of little value. Several good researches on the extent and duration of the elimination of KBr, especially by the urine, cannot atone for the absence of any philosophical groundwork for what is maintained in this work, in a theoretical and practical sense, respecting the action of the bromides.

G. Amburger, a practical physician in St. Petersburg, wrote (on the occasion of a "promotion" in Dorpat), a doctoral dissertation "On the hypnotic action of KBr" (1872), in which he and others relate experiments made in the manner of Laborde. He and some other physicians took pure KBr in doses of 62 to 218 grains, at various times of the day. I will only describe the experiment with 218 grains. This quantity was taken between 2 and 3 P.M., with a little sugar and water, a light *déjeuner* having been taken at one o'clock. During the following hours there occurred—salivation, eructation, headache, nausea, pain in the loins, feeling of heat in the face, small and slow pulse, disposition to urinate, colic, some diarrhœa, difficult breathing, a feeling of anxiety, depression, &c.; but yet, at 11.30 P.M., still no sleepiness, and only at 1 A.M. sound sleep lasting eight hours, which of course would naturally occur about this time without any KBr. On the following day there were still coated tongue and anorexia.

The six earlier experiments, with smaller doses, were almost completely negative. The experimenter found himself, soon after meal-time, always in a dimly lighted room, and in a con-

venient posture, half-clothed upon the bed: the day's business was over, the ordinary hour of repose not far distant. All the conditions were therefore favourable to the expected action of the bromide. But it happened here, with men, precisely as in my experiments on animals.

Dr. Amburger says that the action on the heart began very quickly. Usually the retardation of the pulse lasted throughout the following day.

Against the unconditional value of these researches with negative result, it may very justly be urged that other medicines, also, have no action on the healthy body, while they have the most decided influence on diseased persons. The hypnotic power of KBr may exist under similar conditions. This point, also, is brought to a test in the following therapeutical researches.

By most authors KBr is especially recommended for nervous insomnia. Delirium tremens, hysterical insomnia, and excited conditions of the brain of a primary kind, not caused by bodily pain, fever, or cough, make up a large contingent of cases in which it is indicated. Amburger enumerates thirty cases, of which eleven were delirium, and the rest insomnia of the most various forms, but the majority with the understood character. In pronounced delirium tremens, KBr, even in doses of 110 grains, was altogether ineffective, while opiates or chloral succeeded: in slight cases an improvement was several times observed after the use of KBr. In mental diseases no effect was verified. Of the remaining fifteen cases, five showed good results; in one of these evidently through the sedative action upon the heart, in insufficiency and obstruction of the aorta. The dose here was 54 grains daily. Arguing from the fact that sleep may be produced by psychical influences unaided, he gave in seven cases a sleeping powder, composed of 8 grains of cascarilla bark, and as much sugar. Cascarilla bark certainly contains about 1 per cent. of ethereal oil; but except by persons with fantastic notions about drag-action, little difficulty will be felt in assuming that this was an *inert* powder. Of the seven patients, no less than five fell asleep after taking this inert powder. These cases were (respectively) declining pneumonia, climacteric troubles, over brain-work, convalescence from typhus, and insomnia without recognisable cause. Sleep did not occur

in one individual who had indulged first in pious fastings, and then in alcoholic excess for some days; nor did it occur in a man of fifty-four years with rigid peripheral arteries. Dr. Amburger comes to the following conclusions:—1. That there is no hypnotic power peculiar to KBr. 2. If sleep follows upon the use of KBr, it is with high probability to be ascribed to mental influences.

It will be seen that this experimenter expresses himself still more negatively than do the results of my own researches and those of Eulenburg and Guttman upon animals. We meted out justice to the potassium, however; for in this point the cornerstone of the whole question appears to lie. The potassium-salts are of the greatest importance for the red blood-corpuscles and the general muscular system; the formation of which is impossible without them. According to this, which we have especially learned through the weighty researches of Kemmerich upon these salts, it is very probable that under certain circumstances a more plentiful supply of potassium improves the nutrition in the manner specified, and rectifies the consequences of a chlorotic condition of the blood—viz., an enfeebled activity of heart and blood-vessels. It is further probable that large doses of an easily digestible potassium-salt may directly influence the human heart also, in various senses, and that by these means those abnormalities in the distribution of the blood are rectified which would be reflected from the brain in the shape of insomnia and restlessness. It may further be assumed that in men, also, large doses of KBr act, in Meihuizen's sense, upon the reflex organs, or upon other motor apparatus, in a sedative manner. But we have nowhere verified, so far, a rational basis for the therapeutic activity of KBr in such cases; let it therefore be a theoretic possibility that accidentally the bromine compound of the alkali in question may best and most easily pass into certain tissues, and there vicariously establish itself.

Further, this consideration speaks in favour of the potassium, that there is very little said, in the practical world, about the administration of the other bromides. Among various more or less reserved statements I will only select the latest, since the author, Dr. Hollis, also experimented on himself. He is an adherent of bromo-therapy, and gives a little paper "On the value

of sodic bromide as a nervous sedative.”<sup>1</sup> In the evening he took 22 grains of NaBr, in two doses, during half an hour, upon an empty stomach. No effect on pulse or temperature. At the ordinary bed-time he felt pleasantly sleepy—that is surely the usual case with healthy men?—and slept very well, although he was usually in the habit of waking several times in the night. On the following morning there were deafness and tingling in both fore-arms and in the right leg. Three cases of epilepsy, treated with Na Br, are then brought forward. The first improved after a long course of Na Br; the attacks became much milder; but the patient was taking succus conii at the same time, and had a recent seton in the nape of the neck. The second case relates a distinct success in a boy aged 14: but what agent is there which, in the years of commencing puberty, has not cured all kinds of things? The third case (cure of a man aged 25, who at the time was having five fits daily, by the long-continued administration of 15 grains three times a day) certainly excites our astonishment, the more so because the whole story is told in a little more than four lines. Further, Na Br proved beneficial in “nervous excitement,” was of no use in epileptic vertigo, and was directly injurious in the sleeplessness of an old man.

It is not by such clinical histories as these that anyone is likely to be made enthusiastic about bromide of sodium.

The latest favourable testimony for KBr is that of Senator, who, in his richly-stored book<sup>2</sup> “On the febrile process,” speaks as follows:—“There is only one potassium-salt from which, according to my experience, we may with security expect a direct and most desirable action in febrile diseases, viz., the bromide of potassium, which, appropriately administered, subdues the insomnia and restlessness better than any of the medicines which are ordinarily given.” This author is accustomed to the exact method, and we have no grounds for doubting the correctness of his observation. However, even here the question remains open as to the specifically active element of the drug.

Since we know, from Saikowski, that the discharge of potassium-

<sup>1</sup> *Practitioner*, London, Aug. 1873. Compare also Decaisne, *Arch. Gén.* 1870, p. 760.

<sup>2</sup> Berlin, 1873. Cf. p. 207.



salts is quite specially increased in fever—Senator attributes this chiefly to the increased destruction of the red corpuscles—it is natural to think first again of this metal (potassium), the replacement of which may perhaps be most easily effected by KBr. Senator spoke formerly only of the nitrate and the citrate of potash. It would be important to discern how KCl and NaBr acted under like circumstances. For the rest, if, as is said (at p. 206), there is no need for artificial administration of potash salts, since the milk and broth that are recommended themselves contain sufficient potash, yet this argument is not admissible without something further. Many chlorotic patients with tolerable appetites and good digestion, take sufficient quantities of iron in their meals, yet the pale condition of the blood is only lost when iron is introduced into the alimentary canal *in excess*. Upon such an argument a characteristic representative of the Vienna school, which then (twenty years ago) denied everything in therapeutics, declared that iron was altogether useless. But this theory is antiquated.

If the view advanced by me concerning KBr seems plausible, we might inquire whether iodide of potassium acts in a similar manner. Yet things here lie in a different fashion, Upon the destiny of KI in the human organism we possess almost no researches; but, on the other hand, by means of well-known chemical inquiries we are justified in the following conclusions:—

In the healthy stomach one part of the ingested KI is changed to iodide of hydrogen ( $KI + HCl = KCl + HI$ ); another portion is converted, by the presence of chloride of sodium, into iodide of sodium; a third part remains, if the dose be large enough, unchanged. All these three compounds naturally pass rapidly into the circulation. The HI meets with disposable alkali, and again forms NaI. In the tissues, however, which are in a state of active change (*arbeitenden Gewebe*), the two alkaline compounds of iodine can have no existence. Schönbein has already shown that they are decomposed by the active oxygen. The same thing must also happen in our glands, &c.; since, under conditions which are of a far less energetic nature, it does occur, as the following research shows:—

A diluted solution of pure KI was exposed for about fifteen

minutes to a strong stream of carbonic acid, then mixed with starch-paste, and divided into two equal portions. To the one portion of test freshly prepared water containing protoplasm<sup>1</sup> was added; to the other an equal quantity of ordinary water. In the first test, with a good arrangement of the experiment, the blue colour of iodide of starch was immediately developed, or showed itself within a few minutes. The indifferent protoplasm of the plant-cells had then, with the assistance of carbonic acid, set the iodine free. The acid alone did not suffice, as was to be expected beforehand.

The experiment succeeds still better if, instead of carbonic acid, we employ a very diluted mineral acid; preferably phosphoric acid. Here the acid acts on the circulating KI or NaI simultaneously with the active oxygen of the gland-protoplasm. The iodine is separated, either directly (according to the formula  $2KI + CO_2 + O_1 = K_2CO_3 + 2I$ ) or, what comes to the same thing, after previous formation of iodide of hydrogen; and the free metalloid thus obtains the power to directly seize upon certain albuminoid bodies to which it has, in general, a great affinity. In this relation of the KI<sub>1</sub> (as the experiment shows us convincingly, and with the clearness of a *schema*) there may also lie an indication for the understanding of certain cures of tumours, of the everyday occurrence of which there is no longer the least doubt; the process of which, however, still belongs to the region of mystery. One can, indeed, on the ground of established facts, carry the hypothesis further, and say that there decidedly are certain specifically constructed groups of cells which easily decompose KI and are then affected, in their own existence, by the free metalloid.

My whole argument respecting the decomposition of KI rests on the assumption that in our tissues the oxygen is present as O<sub>1</sub>. At the present day this is an indisputable fact, on the proof of which I have expressed myself thoroughly in another place.<sup>2</sup> Everywhere, when oxidations take place in warm-blooded animals, this occurs by means of the so-called "active" oxygen (the ozone, O<sub>1</sub> of Clausius). Even Hoppe-Seyler, who

<sup>1</sup> Compare, on this point, my recent researches in Virchow's Archiv. vol. xlv. p. 150.

<sup>2</sup> Berliner Klin. Wochensch. 1872, No. 30.

formerly did not incline to this opinion, has very lately expressed himself in its favour.<sup>1</sup> He maintains the existence of "ozone in the organs," and even admits the condensed molecule  $O_3$ , which is not at all necessary to my hypothesis.

We can less easily entertain favourable ideas of a decomposition of KBr analogous to that which KI probably undergoes. If one carries out a similar research to the above (of course without the starch paste) one finds no trace of free bromine. We cannot be surprised at this. The combination of alkalis with bromine is much more stable than that with iodine. At low temperatures neither nitrous nor hyponitrous acid, nor the latter with some free nitric acid, sets bromine free from a diluted solution of KBr and similar salts, while all these agents are known to effect this easily with KI (only that  $HNO_3$ , if both it and the KI are pure, requires preliminary warming). We have, with these agents, often to do with nascent oxygen, qualitatively just as in the action of the protoplasm. Certainly, we cannot altogether deny the transient decomposition of KBr in the human organism, since we see that in the stomach glands hydrochloric acid is set free from chloride of sodium; which at any rate is an instance of equally powerful chemical change. But it is not likely that this decomposition endures, in the case of the officinal bromides: and the notion of Saison<sup>2</sup> that the breath smells of bromine after KBr has been taken, reminds one vividly of the fire-breathing monsters of antiquity.

The negative behaviour of KBr in the test-tube is thus seen to harmonise with the results of our earlier researches, and of the therapeutical observations of Sander and others. Modern bromo-therapy requires reformation if it is to be legitimised on a scientific basis, instead of being merely empirically accepted. The following questions press anew for investigation:—

1. What is the behaviour of NaBr and KCl in the numerous cases in which KBr is at present considered to be indicated?
2. Are the numerous successful results really to be attributed to KBr as a nervine, and not to the natural decline of the morbid processes, to psychical impressions (Amburger), or to an

<sup>1</sup> Pflüger's Archiv. 1873, vii. p. 415.

<sup>2</sup> Quoted by Nothnagel, Arzneimittellehre, p. 40.

improvement of nutrition by means of the potassium, and other factors which mislead observation ?

The simple assertion—that KBr or any other agent is of use in such and such cases—does no service to the progress of therapeutics. That is just what the homœopathists say, with equal justification, when they cure abscesses with *Silicea*, croup with *Spongia*, insomnia with *Calcaria*—all, of course, in extravagant dilution—and it may be read in the works of Rademacher and his better followers, on every side, expressed with sincere conviction. What right, then, have we to abuse that system, when we blindly give ourselves over to similar logic ?

A therapy which aspires to free itself scientifically from systems of indistinct or incomplete testing must either bring forward long series of observations with corresponding series of check-experiments, or else the theoretical and experimental ground-work of those facts which it views as instances of cure. Whether and when the latter is the best method of testing a particular treatment, we know depends upon the solution of a number of preliminary questions : the former is open to every practitioner who has large and not too scattered materials at his command. And now, without wishing wholly to deny the justice of the prevailing bromo-therapy, it may be well expected, from the manifold contradictory facts, that of the many hundred-weights of this salt which are now yearly expended, a few pounds will soon again be found sufficient for the purposes of scientific clinical medicine.

[The Editor has translated the above paper, with the author's consent and wish, from the *Deutsche Klinik*, a journal very little read in England. He has to apologise for some idiomatic defects in what has been a rather difficult bit of work.]

## THE ENGLISH STAND-POINT RESPECTING THE VALUE OF BROMIDE OF POTASSIUM.

BY DR. ANSTIE.

THE severely sceptical paper by Professor Binz which precedes this article will be read with surprise by most English medical men; but those who know the difficulties of therapeutical inquiry will probably feel that such criticisms have great value in forcing us to scrutinise our grounds of belief with additional rigour. We may certainly affirm that several of the positions which Dr. Binz signalises as dubious or false are in this country habitually taken as true. The best test of the matter is to inquire what is the general impression and practice among the eminent men who are, or have been, physicians to the National Hospital for the Paralysed and Epileptic, where thousands of epileptic patients are treated. Of these one may quote Dr. Reynolds, as a well-known author of standard works on epilepsy.<sup>1</sup> Dr. Reynolds speaks of KBr as if it were the only remedy worth consideration in the direct treatment of simple epilepsy. He says: "It is to be demonstrated, in my opinion, that there is something 'specific' in the action of KBr. Potassium—given as iodide—is without such effect; and bromine—given as bromide of ammonium—has no obvious influence on epilepsy; but in combination, these two elements—bromine and potassium—are of undoubted value." And he sums up the general question as to epilepsy thus:—"(1) The cure of epilepsy is effected by doses varying, for the adult, from 5 to 40 gr., given three times daily. (2) It is not the mere administration of the drug, but its

<sup>1</sup> See not only Dr. Reynolds's article "Epilepsy" in his "System of Medicine," vol. ii., but his article on KBr in the *Practitioner* for July 1868.



presence in certain quantity, that is necessary for a cure. (3) The dose which shall prove curative is not determined by either of the following conditions—sex, age, duration of disease, frequency of attack, severity of attack, or form of attack; but (4) Individual cases differ in some points, of which we know only this, that they are curable by different doses of bromide of potassium. (5) That when not curative it is of great value in diminishing the number of attacks; and that the dose in which it produces this effect varies between the limits I have mentioned. (6) That the number of cases in which it proves of no service at any dose is very small; and that the cases which resist its action do not differ in any other obvious respect from those in which the bromide is highly efficacious. (7) That bromide of potassium does no harm, even when given in the largest doses I have mentioned, for it may be taken for many months, and even for years, without producing derangement of any sort or in any direction. (8) That the rash, or acne on the skin, which is occasionally seen, is not determined by the quantity of bromide that is taken. I have seen it after a few doses, of 5 grains each, have been administered; and it has been absent in many cases where 30 grains have been taken, three times daily, for periods of 6 or even 12 months. I would therefore earnestly recommend that bromide of potassium should not be discontinued in the treatment of a case of epilepsy because of its apparent failure; but that the dose should be gradually increased, and the exhibition of the drug most patiently carried on for a period of many months or even years.” Elsewhere Dr. Reynolds remarks that, while thus remarkably efficacious in *epilepsia gravior*, the bromide scarcely at all affects the attacks of *petit mal*, and is also less efficacious than usual where the convulsive attack only occurs at night. In regard to insomnia, this author mentions the sleeplessness of acute mania as sometimes, and that of delirium tremens as often, successfully combated by the bromide. He speaks, also, of paroxysmal (non-organic) vertigo not accompanied by obvious spasm, and paroxysmal headache with flushing of the face, as often rapidly relieved by it.

Dr. Reynolds's is probably the strongest positive opinion as to the value of KBr which has been published in this country; but

there has been abundant testimony from his former colleagues, and from other physicians of large experience in nervous diseases, in support of the unique value of KBr in epilepsy: to this effect I might quote the statements both of Dr. Radcliffe and of Dr. Ramskill; and, indeed, no person who watches the practice of the Epileptic Hospital for a time can fail to perceive that all the officers of that institution rely upon this remedy with a perfectly different kind of confidence from that which they repose in any other drug. Speaking, to-day, with Dr. Hughlings Jackson, who is also a physician to that institution, I carefully compared my own experience (at the Westminster Hospital and in private practice) with his: and we came to substantial agreement on the following conclusions:—

As to epilepsy, we agreed that neither of us had seen a “cure” in the more ordinary sense of the word; *i.e.* we had not observed cases<sup>1</sup> in which well-developed fits had been stopped completely, and had remained absent long after the discontinuance of the medicine. On the other hand, we had both seen numerous instances in which very long periods of freedom had been obtained, on condition of steady perseverance in the use of the bromide, with only short intervals, and the observance of proper dietary precautions. And neither of us felt any doubt that this salt at least reduces the frequency and severity of fits in all but a very small number of cases.

As regards insomnia, both Dr. Jackson and myself felt more hesitation. In delirium tremens Dr. Jackson does not use drugs, but I often use chloral, and in a certain number of instances I have preferred bromide of potassium. I cannot

<sup>1</sup> The solitary exception, within my own experience, is a case so peculiar that I should like to mention it here. John Moss, a boy aged seven years, was admitted under my care into the Belgrave Hospital for Children in 1868, suffering from severe and frequent epileptic fits for which no peripheral cause could be detected, and which were rapidly reducing him to a state of semi-idiotcy. Thirty grains of KBr, daily, stopped the fits completely, and under the kind watching and encouragement given by the lady superintendent the boy's intelligence returned, and at present (for I saw him only a short time since) he is rather above the mental average of his class. There were singular points in his history, after the cessation of the fits. He had a violent right supra-orbital and temporal neuralgia (at eight years of age) ending in erysipelas (distinctly *not* shingles) of the parts: and once or twice he has had threatenings of chorea. All these nervous ailments appeared to be controllable by the bromide in a remarkable manner.

doubt at all that twenty-grain doses of the latter, repeated every two or three hours, have frequently both checked *actual* delirium and induced sleep, and have also averted an impending outburst of alcoholic delirium: and this was especially obvious in alcoholic patients who had tendencies to epilepsy. But as regards more miscellaneous forms of insomnia, I am in much greater doubt, and was not surprised to find that Dr. Jackson, also, was not quite convinced of the direct hypnotic influence of the bromide. This, at least, I can say, that the insomnia of *aged persons* has always appeared to be aggravated rather than relieved by KBr, in my own practice: and as regards sleeplessness from emotional causes (with the exception of the insomnia produced by sexual "excitement with exhaustion"), I am not convinced, on the whole, that KBr is distinctly remedial, and should be inclined to admit the possibility that (as in Amburger's cases) the only effect it exerts is a mental one. I have myself put KBr quite lately to a somewhat searching test. After a night mostly spent in watching a dangerous case of nervous disease, I took sixty grains of KBr with the hope of securing two or three hours' repose in the early morning, but the attempt was quite vain. Unless I am much mistaken, the bromide only aggravated my restlessness, which it is fair to say, however, was perhaps increased by a certain amount of indigestion from having eaten dinner as late as 9.30, and drunk strong tea afterwards. This is not the only occasion, however, on which I have personally taken bromide as a hypnotic without success: and, in my case, it is striking to observe the uncertainty of the bromide as compared with the speedy and absolute effectiveness of chloral hydrate. And the few trials I have had any need to make upon myself are fully confirmed in result by that of the miscellaneous cases of insomnia in my practice. My opinion in this matter, however, would be quite overborne by the mass of English medical authority: and I confess to distrusting my own distrust when I remember the strong testimony of so able an observer as Dr. Warburton Begbie,<sup>1</sup> who makes most important statements as to the value of bromide in the sleeplessness which occurs during convalescence from acute diseases, or after surgical operations. And the general body of English practitioners

<sup>1</sup> Edinb. Med. Journal, Dec. 1866.



certainly have a firm belief in the efficacy of the bromide in the majority of cases of insomnia of whatever kind ; though they would acknowledge that a few individuals are insusceptible of its influence.

From our large public lunatic asylums a great deal of evidence has come in favour of the hypnotic and also of the anti-epileptic virtues of KBr, and it is difficult to believe that here any very serious mistake has been made, as the field of experience has been enormous, and the medical officers of these establishments are, for the most part, men of high intelligence. Perhaps the most carefully executed research on the action of KBr in mental affections is that of Dr. Clouston,<sup>1</sup> formerly at the head of Westmoreland Asylum, now the Medical Superintendent of Morningside, Edinburgh. The following is his summary of the results of experiments on twenty-nine insane epileptics.

1. All the cases were of old standing, all had the same diet, and all, before treatment, had their average number of fits and every necessary particular as to their health of mind and body carefully noted. They had increasing doses of bromide up to 50 grains three times a day ; thirty-eight weeks of treatment in all, during which accurate notes were taken. 2. The total number of fits gradually fell to one-sixth of their average number without medicine. 3. The fits during the day were lessened to about one-twelfth, and those during the night to about one-third of the normal number. 4. The reduction was not uniform in all cases. In one case it was 24,000 per cent., in one-half more than 100 per cent., and in five cases there was no reduction at all. 5. In one-fourth of the cases the fits were much less severe ; in some of these they were as frequent as before. 6. In one-fourth of the cases the mental state was greatly improved. Irritability and tendency to sudden violence were wonderfully diminished where they had been very bad. Attacks of epileptic mania were diminished. In some cases the mental state was improved, while the fits were as frequent as ever. 7. Most of the patients gained considerably in weight while the doses were under 105 grains daily. Their aggregate weight was greater at the end of the thirty-eight weeks than it had been to begin with, but began to

<sup>1</sup> Journ. Mental Science, Oct. 1868.

fall after the above dose had been reached. 8. The temperature fell somewhat until 150 grains daily was reached. 9. The pulse gradually fell about seven beats up to 120 grains daily. After that it rose, but not up to its usual standard without medicine. 10. None of the patients suffered in their general health except five. All the others were benefited in some way, except one. 11. The ill effects produced in the five cases were torpor of mind and body, drowsiness, increase of temperature, loss of weight, loss of appetite, and in three slight double pneumonia. 12. The cases most benefited were very various as to the causes, number, and character of the fits, and in every other respect. On the whole, those with the most frequent fits benefited most. 13. All the patients to whom medicine proved hurtful had had fits from childhood. They were all very demented, and had more than one fit per week, but had nothing else in common. 14. All the good effects reached their maximum in adults at 30-grain doses three times a day; more than this seemed to do some harm. 15. There seemed to be no seriously ill effects in twenty of the cases from 150 grains daily for ten weeks. 16. When the medicine was entirely discontinued in all the cases, the average number of fits increased in five of the cases benefited to, or beyond, their original number in four weeks; in thirteen cases it remained considerably less. The total average during that time was a little more than one-half the number of fits without medicine, and the greatest number of fits occurred in the second week after the medicine was discontinued. Independently of epilepsy proper, we have the valuable authority of Dr. Hughlings Jackson for the statement that the bromide is exceedingly useful in convulsions of children without assignable cause, unless acute anæmia be present.

In spasmodic diseases like false croup and asthma, we have again the very high authority of Dr. W. Begbie for the statement that bromide of potassium is a powerful remedy; and this is backed by the testimony of Ringer, who also adds that it is most efficacious in the colic of children, and also in the night terrors of young children. The latter writer expresses the following opinion as to its effect in convulsive diseases:—“Although convulsions may be excited by many causes, it is probable that the conditions of the nervous centres producing

the attack are in every instance identical; and it appears to be these conditions which the bromide controls."

Of the use of KBr in neuralgia I can venture to speak confidently from my own experience. It is singularly efficacious, but in a comparatively limited number of cases: the majority of these are instances of some form of sexual worry; the remainder are examples of pain apparently excited by anomalies in circulation in subjects who at other times perhaps owe their neuralgia to different causes.

In the above summary of English opinion I have merely touched upon those points as to which there is not only a large agreement as to the positive qualities of KBr, but an agreement among the highest authorities on the respective diseases. I leave wholly unnoticed here a multitude of uses of KBr as to which there is less agreement or less weight of individual authority; for I conceive that if only thus much were fairly made out, it would be scarcely possible to exaggerate the value of the drug. Let me just add that there has been a very general consent among the most experienced physicians, since the publication of the important papers of Dr. Clouston and Mr. Kesteven, that large doses—something like Dr. Clouston's standard—are necessary to produce the more striking results of KBr.

I think that the most sceptical experimenter can hardly refuse to allow that this mass of testimony is not easily to be put aside so far as KBr itself is concerned. It is equally certain, however, that the case, from English authority, is infinitely weaker as regards the other bromides. And this brings us to consider after all what is the central point of Dr. Binz's sceptical inquiry; viz., whether in fact KBr does not act merely in virtue of its qualities as a potassium salt.

Now, in the first place, we are clearly in a position to assert that it is not every potassium salt which will produce effects in the least resembling those of KBr. So far as regards the bicarbonate and the nitrate, I can myself adduce a long and entirely negative experience, having given these salts a most patient trial in several cases of epilepsy without ever having a single gleam of success. As regards KCl the case is different, for I believe very few experiments have been made with that salt;

and yet I notice that Ringer states that its efficacy has been denied, and I am myself conscious of having read English evidence to that effect, although I cannot now quote it. I do not believe, however, that a patient trial, on a wide enough scale, has yet been given to the chloride, and this it seems to be our bounden duty to do, whatever our expectations may be as to the results. We might also go further, and say, that the failure of KI to produce similar results to KBr does not seem in any way to exclude the possibility that potassium is the real active element in the latter. When we turn to the other side of the question and inquire for the evidence as to the activity of bromine, we are met, first, by the already mentioned comparative weakness and isolation of the statements in favour of the other bromides. The next question suggested by the clinical evidence already given is whether the bromides of sodium and ammonium agree with KBr in their "extra effects," viz. in the production of those toxic effects which sometimes so disagreeably complicate the use of KBr. It has been broadly asserted that they do not, but, I believe, without sufficient grounds. I can, at least, affirm that the acne-like rash, and also the mental confusion and loss of memory which KBr is well known to occasionally produce, have been seen by me, on more than one occasion, as the result of bromide of sodium; and that the mental confusion has been produced within my knowledge by bromide of ammonium. I have certainly never witnessed those more seriously inconvenient results first noticed by Hammond, viz. positive though temporary paralysis of the extremities, except as a result of KBr: but this may well be because I have used the latter in a far greater number of cases and for much longer periods of time. It is of course open to Dr. Binz to observe that it is not the toxic effects as to which we have to inquire, and that it may well be that these indeed are due to the bromine, while the therapeutical effects are altogether due to the potassium. To this idea we may oppose the fact, *quantum valcat*, that some of the most marked cases of success with KBr in epilepsy have been distinguished by an uncomfortable amount of these toxic symptoms.

When we examine the constructive part of Dr. Binz's paper, we find him suggesting, first (as already said<sup>1</sup>), that potassium is the important element of KBr, and then, that it may produce



its effects in two ways—first, as a restorative having special influence upon cell-life, and particularly the formation of the blood-corpuscles; and secondly, by its depressing action upon the muscular force of the heart. In support of the first of these supposed functions, it may certainly be alleged that KBr, to a degree that is equalled by but few other drugs, requires to be administered with a persistency and in such large quantities as remind us of the properties of a food rather than of a medicine in the ordinary sense of the word. Dr. Binz himself appears to suggest that the large quantity of potassium which is thus taken is directly valuable; that it is in the nature of a useful excess, such as occurs with other well-known remedies with which one must (so to say) *flush* the organism before one gets any benefit, though theoretically a much smaller quantity should suffice. If that were so, however, I do not see why the bicarbonate should not answer the purpose; but I have several times given that salt to the extent of keeping up alkalinity of urine for as long as was consistent with safety, without producing the least effect on the course of epilepsy.

It is to be remarked that Dr. Reynolds was very clear in the statement of his opinion that the action of KBr is neither that of bromine nor that of potassium, but a specific something produced by the compound of the two. I believe he would now admit that a similar though somewhat weaker influence is exerted by NaBr; and although Dr. Binz has rather severely handled the evidence to the same effect which Dr. Hollis brought forward in this journal, I do not think it will be found possible, in the end, to exclude NaBr from the position of a direct anti-convulsive remedy, acting, so far as its powers extend, on the same lines with KBr. Here, again, as in the case of the potassium-salt, it is absolutely necessary to give large doses to produce decided effects: yet we can hardly suppose that large quantities of sodium can produce any useful results in the direction to which Binz's interesting suggestions point. I would mention, as necessarily of high importance, the opinion of Dr. Weir Mitchell: this distinguished neuro-pathologist finds both the potassium and the sodium salt very useful in epilepsy; but he further states that there are some cases in which neither of these is effective, but the bromide of lithium will produce the desired

result. On the other hand, the bromides of calcium and magnesium, and bromine itself, failed to control the epileptic fits. This is surely a very singular observation, and coming from Dr. Weir Mitchell it assumes an importance which can hardly be exaggerated. For want of space I cannot now indicate the manner in which it appears to me to affect the bromide question. Enough to say that it fully justifies scepticism on that question, in the proper sense of the word; viz., it allows and indeed renders necessary a spirit of doubt and inquiry as free as that of Dr. Binz himself. The question cannot be allowed to rest in its present obscurity: in fact, the very singularity of the nature of the conflict of opinions is almost a guarantee that we shall shortly be enabled to realise Dr. Binz's just aspiration for a scientific basis for the use of KBr.

I cannot close this paper without quoting a remark which may tend to show, better than anything cited above, the strength of conviction with which KBr impresses some English physicians by its effects in epilepsy. I lately asked one of the shrewdest and most experienced of our authorities on nervous diseases, what he thought of the results of the introduction of KBr? He replied: "It has changed the whole prognostic significance of epileptic attacks."



## ON THE ANTIPYRETIC ACTION OF QUININE.

BY DR. CLIFFORD ALLBUTT.

THE progress of our knowledge of the nature of fever and of special febrile disorders, is not the least encouraging of the chapters of modern medicine. Beset and obscured by false hypotheses and its discussion impeded by false terms, the subject of fever was one of the most backward parts of our nosology; but since we have found that fever lends itself to direct investigation by physical methods, and even to quantitative estimation, we have been able of late to put it among the most forward parts of our science. We have made it a matter of common knowledge that fever is a disturbed balance, and that, like other disturbances of a moving equilibrium, it tends under favourable circumstances to resolution.

In particular the loss of balance is seen in a disturbed relation between the formation and loss of heat, and the *vis medicatrix naturæ* consists in the tendency to restoration of the normal balance as it is restored in a spinning top, after a fillip of the finger. We have to estimate the kind and degree of the disturbing force and its mode of application, and we have to estimate the spin or vitality of the system disturbed, and thus, if all the facts can be calculated, the result may be predicted. Many fevers arise from a single interference, which is not repeated, and the initial velocity and the subsequent course of the disturbance depend upon the character of this single interference. Amid much that is obscure we may notice as a rule that disturbances having a high initial velocity are sooner expended than those in which it is lower. Examples of this

are seen in the respective curves of scarlatina, typhus, and enteric fever. In the former two cases, the disturbing cause reaches its maximum almost at once; in the last, as it seems to reach its highest effect by a slower development, so also it recedes more slowly. In other cases of fever we have a constant repetition of the disturbing cause; as in septicæmia or in the lesser varieties of that affection known as hectic. In the former instances we have large and probably frequent absorptions into the system, of material in a state of more rapid combustion than the blood: in hectic we have repeated small absorptions; as in phthisis, chronic diseases of bone, and the like. Clearly, then, there can be no universal antipyretic, but antipyretic methods must include modifications for each of these cases, and probably for others also. Our methods must be founded on a careful estimate of the greater initial velocities of some kinds of disturbance and their earlier resolution, of the more prolonged periods of other kinds with slower resolution, and of the repeated accessions of other kinds in obedience to a recurrent cause. A disturbing cause of high initial velocity and short duration may give us less alarm, and need less resistance, than a cause which will set up a period of disturbance of less intensity but of greater duration. Causes, again, which act less by impressing considerable periods of disturbance upon the system in obedience to one interference, and more by a series of repeated disturbances of short duration, will require other kinds of resistance. In the former cases we have to put the organism in the best position to recover its balance after a single disturbance of tolerably definite extent, in the last we have rather to prevent the repetitions of disturbance. In the former cases we have only to aid the *vis medicatrix naturæ*, that is, to favour the tendency of the moving systems to recover equilibrium and to resolve conflicting forces; in the last case we have less to do with the system and more to do in neutralising the agencies which keep up its oscillations. We must have means at hand, which shall deal with consequences in the former cases and with causes in the latter. The former means would be directly, and the latter indirectly, antipyretic. It seems clear that water baths are remedies of the former class, and that remedies like the sulphocarbolates are credited with virtues of the second class.

Let us now look a little more closely at the problem of directly antipyretic treatment. In this treatment we may aim at two purposes, which again may or may not be quite distinct. We may aim either at a reduction of the intensity of a febrile movement or at an abbreviation of it, or, again, at both objects together. If it be true that to moderate the intensity of a febrile movement, *i.e.* to limit the degrees of its aberration, is also to abbreviate its course—and this, I think, we might fairly have anticipated—then, in attaining the former we should attain the latter purpose also. These questions are not to be solved without extensive and minute clinical research, and this method, so far as it has gone, seems to point out that in specific fevers life is threatened in two chief ways: first, by the direct effects of the primary cause acting in virtue, not of its general quality as a pyretic, but of its specific quality as a poison of undetermined powers;<sup>1</sup> and secondly, by the indirect effects of the same cause acting in virtue of its general quality as a pyretic agent, which quality is common to many, but not to all, animal poisons. The experience of every reader will suffice to prove this distinction; for example, we are often called to rapid deaths in scarlatina, and we find that death within forty-eight hours may occur in two ways which are relevant to our present argument. In one mode of death we find that the primary impulse has driven the balance of temperature beyond the point compatible with continued function; in another mode of sudden death we find that this oscillation has not been excessive, but that some other poisonous quality of the primary cause has arrested life.

Now what is true in these early and striking forms of death is no doubt true as regards death and tendencies to death in more protracted cases. Death may be the result of pyrexia excessive at one time, of pyrexia not so intense at one time but accumulating<sup>s</sup> its pernicious consequences more gradually, or of the specific effects of the poison itself. And I need not say that in many cases death and tendencies to death are due to all these causes acting in various proportion. Leaving out the specific effects of the primary poison, I have at present to deal

<sup>1</sup> How the poison, say of typhus or scarlatina, kills as such seems to be wholly unknown.

only with tendencies to death due to pyrexia, which may be violent, or may be prolonged, or may be both. Its effects of course are much the same, whether these be the result of extreme aberrations for a short period or of lesser aberrations for a longer period. This is not the time to sum up carefully what is known of the effects of pyrexia when thus considered, but I may shortly say that they fall into two chief categories, those seen in discharges, or in oppression of the nerve-centres, and those seen in greater combustion of tissues. This excessive combustion is strongly marked in the muscular tissues, not excluding the heart. Shortly, then, violent or prolonged pyrexia threatens life through the nervous system and through the heart. The degree of violent pyrexia which will kill is tolerably well known, but clinical experience has yet to decide what averages of daily aberration are compatible with the continuance of life for a week, a fortnight, a month, and so on. This ought to be made out. It is certainly true, however, that small gains in daily temperature are very large gains in time, and that as we descend the scale in daily temperature, we get largely multiplied increments of immunity. Thus an aberration of  $107^{\circ}$  is soon fatal, an aberration of  $106^{\circ}$  is alarming, an aberration of  $105^{\circ}$  with slight remittances is tolerable for some days, an aberration of  $104^{\circ}$  with similar slight remittances, may be borne not only longer but very much longer, and aberrations of  $103^{\circ}$  may be borne for a month or more. If there be more oscillation, if, say, the daily temperature fluctuate between  $100^{\circ}$  or  $101^{\circ}$  and  $103^{\circ}$ , the combustion may be tolerated for very long periods, certainly for ten weeks, and perhaps for more than that time. But we need far more careful estimates of this, and such estimates must be made on so large a number of similar patients as to eliminate, as far as possible, the error of varying individual resistance.

One more important point has been raised in a previous page, but has not been considered, which is this—Does an artificial diminution of the degree of the fever on one or on several days modify the after-course of the fever either in its duration or in its average intensity? Observers seem fairly agreed that in fevers which have a definite course it does not so modify it, and in other fevers it is hard to form a judgment. In the



use of any antipyretic agent we have therefore to expect a temporary alleviation only. We cannot shorten the period of the whole aberration, nor can we flatten its trajectory, so to speak, except by a continuous application of our means. We know tolerably well, then, what our aims must be, namely, to stop superheating in the diseases in which this may occur, and in fevers of long periods to flatten the curve so as to bring it within those parallels which experience tells us are safe for its calculated duration.

Let us now endeavour to apply these reasonings. In typhus, scarlatina, pneumonia, and rheumatic fever, the pyrexial danger is generally of the brief and intense variety; in typhoid it is more often seen in the longer continuance of a more moderate daily average; in septic fevers it is seen in aberrations which, although often high, yet so alternate with much lower degrees as to lessen the mean daily temperature, and thus to make them very tolerable to the system. A septic fever, again, is very often added to a specific fever, and may thus either modify its course or may prolong the pyrexia when the more definite movement is expended. Such an event is to be seen in the absorption of septic matter from the throat in scarlatina, from the bowel in typhoid, from the lung in pneumonia, from the skin in small-pox, and so forth. Such septiciæmias, whether complicating or supplementing the course of a specific fever, must be carefully distinguished if our antipyretics belong to two equally distinct and correlative classes.

Before the action of quinine as an antipyretic can be interpreted and rationally applied, we must have something like accurate answers to all the foregoing questions. For my own part, I have nothing to offer but the crude impressions derived from a wide but fragmentary clinical experience. In typhoid fever I have used quinine for about fifteen years, being first incited thereto by Dr. Fuller, at a time when Dr. Fuller was testing the quinine treatment of that fever proposed by the late Dr. Dundas. Soon I extended the use of the drug from that to all other febrile states, and have thus arrived at certain opinions rather than conclusions, which may have some temporary value in the absence of any better results from others. Speaking generally, I have found quinine to be a very powerful

antipyretic in septic fevers. In that kind of remittent pyrexia which is seen in pyæmia, in septic absorption, in erysipelatous peritonitis, and the like, I have generally found that quinine in daily quantities of 20 to 60 grains will greatly reduce the oscillations. I have very often found, indeed, that by its means the rises may be wholly prevented, and the patient made apparently fever free. Again, in the prolonged hectic of pulmonary disease and like affections, I have often found it easy to moderate the daily movements, and perhaps to prevent them; but in such long-continuing cases it is not desirable to add chronic cinchonism to the other symptoms, and five or ten grains is generally the limit of the daily quantity. Little indeed could be gained by wholly reducing the fever; to reduce it in great part suffices to prevent rapid wasting, to prevent chills and sweats, and to restore appetite; but the local disease itself is not removed. Indirectly, of course, we relieve it, inasmuch as the fever and the primary disease act and react upon each other, and the primary failure has more chance of repair if the fever be moderated. But to moderate it seems better than to repress it with a heavy hand, and I have many charts extending over weeks of time in my possession, which show the effect of quinine in bringing the daily oscillations within much narrower parallels, and in thus giving the patient more chance of recovery. Many a case of dangerous pneumonia of the apex and the like has thus ended favourably which looked bad enough until the above means were adopted. Quinine also in doses appropriate to the occasion—and this the thermometer alone can decide—is to be given whenever septic incidents are seen in the course of specific fevers or following them, and if the absorption be slight or temporary, the immediate and repeated use of quinine is invaluable.

Turning now from this free estimate of the value of quinine in septic and hectic states to its effects in specific febrile periods, I speak with less confidence. Sometimes it fails even in very heavy doses to depress the curve at all; sometimes it depresses the curve for a time; but the depression is followed by a bound upwards which the same means cannot control. Such results we generally see in the ascending section of the curve. I have not had much reason to congratulate myself on



the use of heavy doses of quinine in depressing the elevation of the first four days of typhus or pneumonia, or in the hyperpyrexia of rheumatic fever; often I have thought my interference to have been positively harmful, but of this it is difficult to be sure. Therefore, when we have to meet immediate danger from a rapid rise, I put no trust in quinine, but resort at once to cold baths, bags of ice, and the like. If, however, a high temperature is doing harm at the latter part of a period, quinine will often act like a charm. If, for example, towards the end of the third week of typhoid, a temperature of  $104^{\circ}5$  or  $105^{\circ}$  is likely to be too much for the frame already too far consumed to resist it as it would once have done, then 20 grains of quinine is often very valuable; and such doses, repeated as may be required, may determine the close of the period and release the sufferer. In the second week we get no such success as this. Again, after or during defervescence, we often meet with impulsive elevations of the curve, apparently not due to any septic absorption, but which betray the enfeebled tension of the regulating power of the body; against these quinine is most useful, and often cuts them short at once. Their return may also be prevented by the prescription of five grains of quinine every morning for a few days.

It is not easy to frame any general explanation of these facts, but I will attempt it in a provisional way. First of all, quinine is perfectly harmless, and is well borne in fever; I have often given as much as three and four drachms in twenty-four hours, and often in cases where it certainly has exercised its antipyretic powers, it has not produced even cinchonism.

Secondly, its action is more and more useful as the febrile movement approaches the remittent and intermittent forms. In "blood-poisoning," when the temperatures vary between  $101^{\circ}$ , say, and  $103^{\circ}5$ , it is useful, and if the oscillations be between  $99^{\circ}$  and  $104^{\circ}$  it is invaluable so far as the fever is concerned. But the victory has too often seemed to me to be a barren one. If the "blood-poisoning" be comparatively small, the disease may be, or seem to be, cut short or reduced. But if the poisoning be more intense and its sources cannot be controlled, as in puerperal cases, for instance, I have been sadly disappointed to find, although the temperatures may have been kept down even to the normal, that death or great local mischief has scarcely

been warded off. I lately saw a case with Mr. Carter, of Leeds, in which a lady died after some three weeks of septic fever following small-pox, although we were easily able to keep the daily curves within or almost within the parallels of health. Her life was probably prolonged, but not saved. She became weaker, nervous tremors set in as usual, the breathing became more rapid, and she died of exhaustion in spite of unlimited food and alcohol. I saw a case very like it about a year ago with Mr. W. Hall, of Leeds. We kept the temperature down as we pleased, but we could not flatter ourselves that the patient's state was materially lightened thereby. After a long fight her life was spared, but an enormous abscess or abscesses formed in the right shoulder and arm. This was a puerperal case. So that I cannot regard quinine as an indirect antipyretic by virtue of any power as a direct antiseptic, as I was once tempted to suppose. On the contrary, it checks the fever, while a fatal issue nevertheless seems to prove that the septic mischief may, and often does, continue unmoderated. It is almost unnecessary to state how far my experience is borne out by the well-known effects of quinine in intermittent fever properly so called. Once more: is quinine useful in moderating the average intensity of continued fevers having a definite course, such as typhus or typhoid? To use my former figure of speech, can we by quinine flatten the trajectory of such a disturbance; and if so, what do we gain by it? We might fairly hope to limit the injurious consequences of a prolonged pyrexia, such, for example, as the combustion of the heart. My own experience of the continued use of large doses of quinine in typhoid is full of contradictions. On the whole I have not a very cheerful view of our capabilities in this respect, and as a matter of practice, I have found myself neglecting to use quinine at all during the stages of ascent and of culmination, and reserving the drug for the time of flickering when the remittent oscillations of impending lysis enable me to act with certainty if required. But this question demands far more close work than I can pretend to have given it. If we can have the completed charts of at least one hundred cases of young and previously healthy adults, in which quinine has been given from the outset, we may get some more light. Even then we must remember how variously typhoid shows itself. Not

only does it prefer particular families, but it is much modified by the calmness or the irritability of the inherited nervous system. Some patients, again, lose the first sound of the heart more quickly at a given range of temperature than do others, and by large numbers of cases only can we hope to eliminate this source of error.

NOTE.—I do not discuss the question whether quinine acts or fails as an antipyretic in virtue of any relation to the nervous system, or in want of such virtue. I have always felt great hesitation in admitting that fever depends upon the nervous system in any essential way, and have always regarded the regulating centre as a very doubtful speculation. That the nervous system is largely influenced in fever, and that by its means the effects of fever may be more widely and rapidly felt, is tolerably certain, but all the elements of fever must be independent of any nervous system at all. If I may judge from the syllabus of Dr. Burdon-Sanderson's recent lectures, he is of like opinion with myself.

## THE VIVISECTION CONTROVERSY.

BY THE EDITOR.

It is greatly to be regretted that the subject of vivisection should have entered upon the stage of clamorous discussion in the public journals, from which nothing but evil, at any rate for a long time to come, can possibly result. For those who cynically enjoy every fresh illustration of the fact that one silly person can do more harm in a day than ten wise men can repair in a year, there may be choice recreation in this onslaught of the pseudo-philanthropists: but for anyone who desires that truth and common sense may prevail, there is nothing but weariness and disgust in the spectacle. Meanwhile, there is at least one duty which appears to us to be incumbent upon the conductors of all medical journals: namely, to protest in decided terms against the conduct of medical men who publicly put forward reckless statements against the practice of vivisection, and make hideous accusations against physiologists of the highest character and reputation, the said statements and accusations being such as will not bear a moment's calm inquiry. Mr. de Noe Walker is presumably a young man, and the temptation of occupying a large space in a public sensational discussion has probably been too much for his common sense as well as for his sense of justice and propriety. But nothing that can be urged in his excuse forms any reason for permitting him to circulate statements which are in the highest degree incorrect and mischievous. We shall therefore make a direct answer to his principal allegations. In the first place we may notice his statement that vivisection has not been of any real service to physiology, but, so far as it has had any influence at all, has been deleterious

and misleading. To this one can only say that it is not possible to open any standard text-book of physiology, and turn to the exposition of any of the more important principles, without discovering that the first sound and sure information on the subject was obtained by means of experiment on living animals. The other charge by Mr. Walker which requires notice is far more serious. It amounts to the statement that the representative physiologists of Europe generally, and Professor Schiff in particular, are guilty of gratuitous cruelty to animals: and as regards Schiff, the statement is backed up by a story which, in its (doubtless unconscious) *suggestio falsi*, is more blameworthy than anything which we have ever seen stated by any person who has enjoyed a scientific education. The story is, that the man who purchases the skins of the defunct animals from Schiff's laboratory declares that nowadays these skins are so hacked that they are valueless; the inference being that the physiologist has performed repeated cutting operations on the animal during its life. It is needless to say that (as Mr. Ray Lankester has already pointed out in the *Times*) this hacking must have been the work of a careless dissector after the animal was dead: and that no such "hackings" are done on the live animals, as physiological experiments are made with but limited incisions, and for one definite purpose. And as regards the general charge of gratuitous cruelty to animals, we will simply take three of the most representative advanced physiologists now living, and inquire as to their practice. Schiff himself shall be one, Ludwig another, and Burdon-Sanderson a third. All practise vivisection: and concerning all of them, it happens to be known with absolute certainty that they are most scrupulous in avoiding any unnecessary pain to animals. It is with no little indignation that we perceive that the priceless services which the labours of such men have rendered to physiology during the last quarter of a century can be lightly spoken of, and even perverted into a distressing accusation before the public, by a medical man who, whatever his talents may be, has never won any title to speak in the name of modern physiology, or modern medicine.



## Reviews.

*A System of Midwifery, including the Diseases of Pregnancy and the Puerperal State.* By WILLIAM LEISHMAN, M.D., Regius Professor of Midwifery in the University of Glasgow, &c. 8vo. pp. 831. Glasgow: Maclehose. London: Macmillan and Co. 1873.

*A Manual of Midwifery, including the Pathology of Pregnancy and the Puerperal State.* By KARL SCHRÖDER, Professor of Midwifery and Director of the Lying-in Institute in the University of Erlangen. Translated from the third German edition by CHARLES H. CARTER, M.D., B.Sc. Lond.

THE almost simultaneous appearance of these two important works (for to most Englishmen Schröder is now for the first time accessible) cannot fail to be of interest to our readers. Undoubtedly these text-books represent an advance, by many degrees, from the stand-point of the obstetrical treatises which were offered to the student of thirty years back. The improvement thus represented has been no single man's work; but among the many labourers none have been more assiduous or useful than the authors who are now under our notice.

The work of Professor Schröder has already received such a strong testimony of approval in the fact that two German editions have been disposed of in two years, that we need not discuss every part of it. Its English dress is quite satisfactory, the translation having been done with care and efficiency by Dr. Charles Carter, who is justified in speaking of his labour as "a very arduous undertaking," and who deserves thanks for everything except some slight clerical errors. Among the portions of this book which have more particularly attracted our attention, are naturally those devoted to the therapeutics of parturition and of the diseases of the puerperal state, especially puerperal fever. We have been very much struck with the condensed and yet perfectly intelligible form of the practical directions which Schröder gives respecting all points in the



conduct of labour, whether natural or complicated. The way in which he introduces the first subject (of the *general* pathology and therapeutics of parturition) at once gives one a confidence in the author's grasp of his subject that is particularly desirable for the student of a branch of medicine like obstetrics, which of all things should be taught dogmatically. The only point which has struck us with surprise is the apparent over-timidness which Schröder displays as regards the use of forceps. Surely he somewhat overrates two or three difficulties and dangers. In the first place he deprecates the use of forceps when the head is not yet engaged in the smaller pelvis, and (unless we have misunderstood his meaning) would appear wholly to discourage the application of this instrument when the head sticks as high as the brim; and although this is a question of degree, still it appears very undesirable to exaggerate the dangers attending an operation which we have repeatedly seen performed—with difficulty, certainly, but with perfect success under circumstances of the most formidable character. It is difficult to suppose that we have misunderstood the author's drift, because he expressly deprecates the making of too long forceps, on the ground that such instruments form a temptation to instrumental delivery in circumstances where it cannot safely be done. Another thing which struck us was the strong manner in which Schröder expresses himself as to the danger of such forceps-compression as could at all materially diminish the size of the foetal head by altering its shape. That serious consequences have sometimes followed the powerful compression of the foetal head is doubtless true; but we are convinced that they are greatly rarer than could be supposed on any *à priori* view of the matter. It is impossible to imagine foetal heads more strongly compressed than many which we have seen must have been by the mere act of natural labour in a primipara, with perhaps but very slightly contracted pelvis, but with powerful pains resisted by very slowly dilating soft parts. One of the most extreme examples, indeed, occurred in a case where there was no pelvic contraction whatever: the appearance of the head after birth was alarming, for it looked like an immensely long sugar-loaf, and one could not persuade oneself that such a head could ever come quite right, or that the brain had not suffered so much compression as must render good mental development impossible. Yet at six months old this child had a remarkably beautiful and regularly formed head, and an amount of intelligence far beyond the average. Of course the answer to this argument is, that pressure from resistance of the maternal soft parts, however severe, is comparatively regular and constant, whereas the efforts of a too impatient operator might very probably inflict severe *sudden* pressure with disastrous results. But to say this is to say no

more than might be urged against a score of operative procedures which, nevertheless, no one would think of prohibiting to a surgeon of fair competence and prudence.

The view which Schröder takes of the diseases classed under the general denomination of Puerperal Fever must naturally be very interesting to English medical men, in whose country, as the author candidly admits, the foundations of sound doctrine on this subject were entirely laid. To Denman our author traces the first observation of the essentially septic character of the typical puerperal fever; and his own doctrine, broadly and strongly stated, is, that the septic is the only essential character of the disease. Before we carry our remarks any further, we may be pardoned for indulging in a lamentation on that ever-fruitful source of misery and confusion, the eternal disagreements of pathologists as to the meaning of words. Here is Schröder arguing strongly that puerperal fever is not a *contagious*, though it certainly is a *transferable* disease. Had he limited himself to maintaining that it is not a *specific* disease in the sense that typhus is specific—*i.e.* always propagating itself in the same form—everyone must have agreed with him. But to deny that it is contagious (although it is true that this course is sanctioned by high authorities) seems to us most unreasonable. If there is one thing more than another which would seem to deserve the name of *contagium*, it is a poison which is readily transferable either by direct contact or by emanation within a moderate distance: and this is eminently the characteristic of the puerperal poison. We shall resume the review of these important works by Schröder and Leishman in our next number.

(*To be continued.*)

## Clinic of the Month.

**Diseases of the Chest in Children: their Treatment by Blisters.**—Dr. D. Maclean, of Glasgow, states that diseases of the chest hold a remarkably high place in the yearly bill of mortality; causing in Scotland the deaths of as many children of five years of age and under as the deaths at all the other ages put together. To reduce this mortality Dr. Maclean suggests the application of counter-irritation in the form of small blisters over the roots of the nerves going to the chest, and those auxiliary to the act of respiration. The most appropriate spot for their application is immediately behind the ear. The form of blister he recommends is the *tela vesicatoria* of the Messrs. Smith, of Edinburgh, which may be applied for the space of three hours. Such counter-irritation is only of value in disease attended with a permanent or prolonged irritation of the tissues of the smaller bronchial tubes and air-cells. It is of value consequently in bronchitis, pneumonia, measles, and congestion. Dr. Maclean thinks he can thus shorten the duration of the disease, reduce the length of the first stage, and hasten the recovery. (*British Medical Journal*, Dec. 13, 1873.)

**The Growth of Cicatrices.**—Mr. W. Adams, in a paper read before the Medical Society of London, demonstrates that scars made in childhood grow with the general growth of the body. He exhibited casts taken at different periods of life, in some of which a growth of as much as an inch had taken place in the course of six or seven years. After deep wounds, or when a portion of skin has been destroyed, the cicatrix appears to be persistent through life. Although all cicatrices at the time of their formation are much less than the wounds from which they result, still, if the wounds should be made in early childhood, the resulting cicatrix will be at the completion of growth very much larger than the original wound; but cicatrices of wounds made after the completion of growth maintain through life the same proportions. With regard to the wearing out of scars, Mr. Adams thinks that those scars only wear out which result from superficial cuts which do not penetrate fairly

through the deeper layers of the skin into the subcutaneous fat. (*Ibid.*)

**Esmarch's Bandages for Bloodless Operations.**—Mr. S. Gamgee, of Birmingham, observes that being impressed with, he resolved to test, the value of Esmarch's bandages for bloodless operations. The patient, a youth aged nineteen, was suffering from acute caries of the middle third of the left femur. Chloroform having been administered and the heel well raised, he bandaged tightly, with india-rubber bandages two inches wide, from the roots of the toes, over the heel, to a point two inches above the intended seat of operation. While an assistant held his finger on the uppermost turn of the bandage, the limb immediately above it was very firmly encircled with several turns of india-rubber pipe, about the thickness of the index finger. The ends of the pipe, which was two yards long, having been firmly tied together, the elastic bandage was uncoiled, leaving the limb shrunken and waxy pale. A three-inch incision in the middle line down to the femur, free use of the gouge and chisel in the removal of a quantity of carious bone, an incision into a boggy spot inside the knee, and the passage of a drainage-tube inside the opening thus made from the incision over the middle third of the bone, were the successive steps of the absolutely bloodless operation. The scanty, pale, serous exudation was barely sufficient to impart a light yellow stain to a white handkerchief. The elastic cord once freed and allowed to uncoil, pink suffusion of the previously dead-looking limb was the work of a few seconds. Blood now trickled from the wound, but certainly not more freely than usual after such an operation. Subsequent progress has been most satisfactory. The lad is easy and feeding, the femur is reduced to nearly normal size, the discharge lessening, and the wound rapidly granulating. This success led him to adopt the same method in a case of excision of the knee with the assistance of a colleague, Mr. J. F. West. The proceeding was bloodless, though the hæmorrhage was rather free when the bandage was removed from the middle of the thigh. The small arteries, which spurted out freely, were twisted or ligatured by the resident surgeon, who applied the usual apparatus to secure immobility. The case was proceeding (ten days after operation) quite satisfactorily, with exceptional freedom from pain. (*Lancet*, Dec. 20, 1873.)

**The beneficial influence of Sea Voyages on some forms of Disease.**—In a paper read before the Hunterian Society in November last, Dr. Peacock gives the results of his experience on the cases in which sea voyages may be undertaken with advantage. After mentioning several forms of disease, as the exhaustion which



occurs from overwork, incipient disease of stomach or brain, he goes on to say that a sea voyage may be recommended to young men who, perhaps always delicate, have become seriously out of health about the period of puberty or when passing into manhood. Such persons are pale, thin, and altogether anæmic and ill-nourished, have a feeble circulation, and their appetite and digestion are impaired. They are unduly susceptible of cold, and an ordinary catarrh gives rise to cough, which is not easily got rid of. On examination of the chest, no serious mischief is found, though there may be a general want of clearness on percussion at the upper parts, and of full and free respiration, which are so often the first indications of pulmonary weakness. These symptoms are regarded with more apprehension if other members of the family in any case have exhibited symptoms of delicate constitution, and especially if either parent has died from consumption. In such instances a voyage should be selected which would take the patient away for the winter. For this purpose a trip to the Cape of Good Hope or Natal, and a journey on the table-lands of Cape Colony or the Free States, to New Zealand, or Tasmania, or to different parts of the Australian continent, often answers very well; preference being given to the cooler climates over those which are warmer: and Dr. Peacock thinks he has seen the best results from visits to South Africa. The same purpose may also be well accomplished by voyages down the Mediterranean, undertaken at the same period of the year, the invalid stopping from time to time at different places so as to remain from home during the winter. Voyages to the East or West Indies or to China are less eligible, as are also those to Rio Janeiro. (*Medical Times and Gazette*, Dec. 20, 1873.)

**Rupture of the Aortic Valves by Accident.**—Dr. Balthazar Foster, in a clinical lecture on this subject, gives a series of cases in which the accident occurred. In most of them well-marked symptoms followed. The immediate effect of the rupture is, that the blood pours through the rent into the left ventricle. That cavity, surprised, as it were, by the novel rush of blood from the aorta, in addition to the accustomed stream from the auricle, staggers under the load, falters in its action, and syncope occurs. When the ventricular muscle rallies again to its work, it cannot for a time cope with the increased blood-charge. Whatever flows back from the aorta must keep back some of the contents of the auricle, and so distend that cavity and soon congest the lungs. The ventricle dilates, however, under the extra blood-pressure, and thus throws at each systole into the aorta a larger quantity than the normal charge. In time this extra charge becomes large enough to allow for the regurgitation, and

still keeps up an approximation to the normal contents of the arterial system. For a time a healthy cardiac muscle would be competent for this increased effort, but its reserve power would not last long, and its nutrition would suffer; therefore the next change soon occurs. Not only does the cavity dilate to contain more blood, but the walls thicken so as to propel the large quantity with sufficient power. This hypertrophy renders the left ventricle a more equal match for the valve defect. And when the hypertrophy and dilatation are so nicely adjusted to the lesion that each ventricular systole propels into the aorta a charge of blood sufficient to allow for the reflux without robbing the arterial system of its due, the compensation is practically perfect, and the normal balance between the arterial and venous contents is maintained. Unfortunately this compensation is often only temporary, and Dr. Foster proceeds to give the reasons why it is so. He believes the seat of the rupture has a considerable influence in determining the duration of life for two of the segments. Each have above them a coronary artery which is filled by the blood column as it rebounds from their curtains. When these segments are torn down and retroverted, they must receive less blood, and consequently the heart nutrition must be proportionately impaired. But that segment by whose incompetency Dr. Foster thinks a murmur is especially carried to the left apex, has no coronary artery above it, and therefore, when it is affected, we should expect the coronary circulation to suffer less than when either of the other segments is imperfect, and life in this case is more prolonged. (*Ibid.*)



## Extracts from British and Foreign Journals.

### Experiments on Strangulated and Incarcerated Hernia.

—Dr. F. Schweninger has made some interesting experiments with a view of testing the truth of the theory broached by Roser to the effect that the intestine plays an active part in the production of strangulated hernia and the like. In these experiments the abdominal cavity of dogs was opened by a small incision in the linea alba; a loop of intestine was drawn out and embraced by india-rubber rings of various width; the intestine and india-rubber rings were then reintroduced into the cavity; the wound carefully closed, and the parts examined after the lapse of a variable time. Dr. Schweninger found that the results might be included in three groups.

1. In the first, the immediate cause of the constriction lies in the gut itself: the intestine takes an active part in the process, to wit, that portion of it which is forced through the constriction assumes a club-shaped or clavate form, and strives to stretch beyond and away from the ring, dragging the adjoining parts until, in fact, the hernial sac is completely filled. The process of strangulation is here of a chronic nature, and the parts are most easily reduced. He does not deny that the difficulty in the way of reduction may be greatly increased by Roser's valvular mechanism or by the inflection of the intestine on itself.

2. In a second class of cases the strangulation is due to a want of proportion between the intestine and the hernial opening, though it is augmented by the above-mentioned action of the intestine.

3. The strangulation is caused by the narrowness of the hernial opening, either because the opening is dragged upon by muscular tissue, or because a portion of omentum surrounds the neck of the hernial protrusion.

As a consequence of these conditions there occur (1), venous hyperæmia and ecchymoses, which readily pass into gangrene on removal of the constricting cause, and (2), insufficient supply of arterial blood, which also leads to gangrene or to inflammation.

with formation of adhesions between the intestine and the sac. Such cases can only be cured by operation. (*Centralblatt*, 1873, No. 48.)

**Physiological Actions of Theine, Caffeine, Guaranine, Cocaine, and Theobromine.**—A pamphlet has just appeared with this heading, by Dr. Alexander Bennett, who states that he has performed upwards of one hundred experiments on frogs, mice, rabbits, and cats, with the aid of Dr. McKendrick, in the physiological laboratory of the University of Edinburgh. The conclusions at which he has arrived are as follows:—

1. The physiological actions of tea, coffee, guarana, coca, and cocoa are mainly, if not entirely, due to their proximate principles.

2. Theine, caffeine, guaranine, cocaine, and theobromine are powerful poisons, inducing a series of symptoms affecting the nervous, respiratory, circulatory, vaso-motor, and glandular systems, which terminate, if the dose be large enough, in death.

3. These five principles are to all appearances identical in physiological action.

4. In small doses, not ending fatally, these five substances produce (1), cerebral excitement, not succeeded by coma; and, (2), partial loss of sensibility.

5. In large doses they produce (1), cerebral excitement; (2), complete paralysis of sensibility; (3), tetanic spasms and convulsions; and (4), death.

6. They paralyse the entire posterior columns of the spinal cord, also the entire system of peripheral sensory nerves; but the anterior columns of the cord and the peripheral motor nerves are not paralysed.

7. They frequently produce convulsions of a clonic character, but occasionally they cause tetanic spasms, which latter are sometimes so severe as to cause opisthotonos. There is at first sight a resemblance between these spasms and those following the administration of strychnia. But in the case of strychnia the action of the poison is limited to the spinal cord, the reflex function of which is so much excited that the slightest touch causes powerful spasms. A poisonous dose of theine, caffeine, &c., on the other hand, paralyses the sensory nerves so that external irritations do not affect the cord; but, notwithstanding, there are strong spasms which are probably caused by a stimulant action of the drug on the anterior columns, and which spasms are not to be considered as reflex in their nature.

8. They do not produce muscular paralysis.

9. They at first increase, then impede, and lastly stop the respirations.

10. They at first increase, and finally diminish, both the force and frequency of the heart's contractions.

11. They produce at first contraction, and afterwards dilatation, of the capillaries and small blood-vessels, with stasis of the blood, indicating first irritation and subsequently paralysis of the vaso-motor nerves.

12. They affect the temperature by (1) slightly lowering, and (2) increasing it.

13. They usually produce contraction of the pupil.

14. They produce an increase of the salivary secretion.

15. They induce a peculiar form of tenesmus, accompanied by a copious discharge of clear mucus from the bowels.

**Reduction of large Herniæ by means of India-rubber Bands.**—Dr. John Duncan, of the Royal Infirmary, Edinburgh, states that several years ago he heard M. Maisonneuve, in his clinical lectures, referring to the treatment of large herniæ, suggest an ingenious method of reducing them. When a hernia of large size is incarcerated or strangulated, it becomes a difficult matter to apply the pressure necessary for its reduction. Plainly, the force applied should, if possible, be of such a character that the pressure can be steadily maintained, and must be so adjusted that equal support is given to every portion of the tumour. With the unaided hands this is impossible. One hand must be applied to the neck of the hernia, and under such condition the other is not large enough to encircle the tumour. The force used is consequently unequal at different points, and therefore dangerous and ineffectual. By means of an india-rubber bandage these indications of treatment can be fulfilled. A turn or two of the bandage is first made pretty firmly round the neck of the sac, and then layers of the bandage are placed in succession over the surface. Care should be taken that the first layer be lightly laid on, so that until the whole surface of the swelling is covered, the tension of the band may be very slight; but after the first layer, one or two others may be applied with a firmer hand. In this way only can complete equality of pressure be obtained in every part.

Dr. Duncan gives the details of a case in which he had recently an opportunity of using the bandage with good effect. The patient was an elderly gentleman with a large scrotal hernia. His mental condition (one of restless dementia) was such that no truss could be got to command persistently the large orifice. The bowel escaped from the abdomen perpetually. As the orifice was very large, and the danger of strangulation consequently slight, a scrotal bag was deemed the form of apparatus best suited to the case. The bowel was returned with great ease when the patient lay down. Once or twice, however, it was not so. The tumour got tense and tender, and pain in the abdomen and vomiting

supervened. These attacks would pass off in forty-eight hours if the patient were kept in the recumbent position. On one of these occasions when the duration and severity of the illness were greater than usual, Dr. Duncan was called to see him. The hernia was then found to be as large as a child's head, and very tense. An ineffectual effort was made to reduce it. Its unwieldy bulk gave a feeling that it was impossible to accomplish its reduction. Dr. Duncan therefore procured an ordinary elastic bandage, and, using it in the way mentioned above, at once succeeded in returning the bowel. This method of reduction, Dr. Duncan thinks, is applicable only to large herniæ. In them only can the bandage be applied. But these cases seem peculiarly fitted for the method, inasmuch as the orifice is commonly large, and the condition therefore calling for interference is primarily rather one of incarceration than one of strangulation. A considerable amount of force is justifiable under such circumstances, provided always that it can be applied with the precaution necessary to prevent bruising or other injury of the hernial contents. With the hand this is difficult; with the bandage, easy, and the degree of force used may be estimated by a preliminary experiment on the closed fist. The bandage may be made of simple india-rubber, or of cotton and india-rubber webbing. The one Dr. Duncan used was of the latter material, very soft and elastic. (*Edinburgh Med. and Surgical Journal*, Nov. 1873.)

**Treatment of Strangulated Hernia by Aspiration.**—M. Dubreuil has recently read a report on a paper by M. Dieulafoy on the treatment of strangulated hernia by aspiration. In this he relates twenty-seven cases, showing the great utility of this procedure in many cases, and its innocuity in all. It is true it implies that a diseased intestine may be returned, but the same remark applies to the taxis. M. Dubreuil, in a discussion which followed the reading of the paper, stated that he could not accept all the conclusions of the author. He did not admit that aspiratory puncture should be the first means resorted to, believing that moderate taxis, which is always inoffensive, should first be tried; but he did not hesitate to recognise that aspiration constitutes a real advance in the treatment of strangulated hernia. Both MM. Verneuil and Trélat also admitted that in certain cases aspiration is very useful, as when the strangulated hernia is complicated by effusion into the sac, the withdrawal of this liquid by aspiration much facilitates exact diagnosis, and allows of a hernia being reduced which had previously resisted numerous efforts by the taxis. This can be brought to bear directly on the intestine, which is no longer marked by the presence of the liquid. Aspiration



thus becomes at once explorative and curative. M. Trélat took the same view of the utility of aspiration, but M. Sée stated his opinion that all that is necessary may be effected by means of a trocar. M. Vernueil pointed out that the evacuation of the liquid by a fine trocar is a matter of difficulty, whilst it is easily accomplished by aspiration. M. Després, the irreconcilable adversary of aspiration, predicted for it a similar failure. In his opinion there is no other treatment for strangulated hernia than operating. (*Medical Times and Gazette*, Nov. 22, 1873.)

#### Facial Neuralgia treated by Resection of the Nerves.

—Dr. M. Schuppert records two cases in which he successfully adopted this practice; in one, the patient was thirty-one years of age, and suffered from neuralgia of the right side of the face, for which he had all his molars extracted successively without relief. The hard palate was then pointed to as the seat of pain, and Dr. Schuppert scarified the outer and inner surface of the posterior portion of the upper maxillary bone, with the result that the pain was removed for some months, when it returned with all its former severity. It started on this occasion from the right canine tooth, which he also had extracted without benefit. He suffered great pain for the succeeding nine months, when Dr. Schuppert resected the infraorbital nerve from its egress at the foramen infraorbitale to the anterior dental nerve, including the latter in the operation. The only results obtained were a perfect anæsthesia of all the parts of the face supplied by the nerve, and a partial loss of the mobility of the corresponding muscles. The neuralgia remained untouched by the operation. Induced by the happy results obtained by Dr. Carnochan, of New York, from resection of the trunk of the second branch of the fifth pair of nerves, Dr. Schuppert determined to have recourse to this operation. By accident he was led to execute the resection, not as Carnochan proposed, from below the eye, taking the infraorbital nerve as a leader, but from the supramaxillary fossa, a method of operation not hitherto performed. To reach the nerve in that locality, portions of the malar bone and of the zygomatic process of the temporal had to be removed. The small space in which the nerve was imbedded was enlarged by cutting away a portion of the posterior wall of the superior maxillary bone. The nerve was divided behind the posterior dental nerve, and by a second cut was removed from the foramen rotundum. No pain recurred for two months, and the case was published as having resulted in perfect recovery. The pain, however, then recommenced, starting from the right-hand palate, spreading from thence up to the eye and temple. The most remarkable feature of the case

was the perfect restoration of the sensibility of the parts. The patient asserted that with the restoration of sensibility the pain had also returned. Dr. Schuppert was aware that, notwithstanding the two preceding operations, a portion of the nerve had been left behind. In the first resection, the main nerve had been divided just behind the anterior dental, and in the second behind the posterior dental, and he therefore resolved to remove the intermediate stump. The stump was found in the middle of the floor of the orbit, with branches attached to it belonging to the middle dental, after the division of which it was easily removed, measuring 15 mm. in length ( $\frac{3}{8}$  inch), but with no newly formed nervous matter either before or behind it. To assure himself that the nerve was wholly destroyed, the actual cautery was pushed beyond the posterior wall of the maxillary bone, the eyeball being protected. After the wound had been closed with the exception of its lowest part, in which a drainage-tube was inserted, a general rigor occurred, lasting for nearly an hour. On waking from the chloroform, intense pain in the head was experienced, which lasted for four hours. The next morning the patient's face was considerably swollen, and he complained of the old pain, so that this severe operation was attended by a completely negative result. In the course of a fortnight the pain, which now included the parts supplied by the third division of the fifth, was so unbearable that the patient begged that some further operation might be undertaken. He was again placed under chloroform, an incision was made, beginning below the lobe of the left ear, and carried down the edge of the lower maxillary bone: the ordinary proceeding for exposing the pterygoid muscles in dissection was then adopted, and the inferior dental nerve and artery were then divided, and a piece of the former an eighteenth of an inch in length removed. On recovering from the anæsthesia tormenting pain was still complained of. Large doses of quinine and opium relieved it to some extent, and the acute sensibility of the lower part of the face was abolished. He returned to his work; about a year later, however, he reapplied for relief, and Dr. Schuppert was forced to the conclusion that the seventh must be the nerve implicated. Still, on reading a paper by Nussbaum to the effect that ligature of the carotid was the *non plus ultra* against facial neuralgia, he preferred to try that plan rather than re-sect a nerve believed to be purely motor. This was accordingly done, but again without relief, and the division of the seventh nerve was finally agreed upon. After a long, tedious, difficult, bloody, and dangerous operation, that nerve was divided. Serious symptoms followed, but the pain was gone. The regular application of the electric current was commenced, as paralysis of the muscles of the left extremities had supervened. The paralysis



was permanent in the upper extremity, but partial recovery took place in the lower. No return of the pain has occurred during the last five years. (*New Orleans Medical and Surg. Journal*, Nov. 1873.)

**The Pathology and Treatment of Typhoid Fever.**—Prof. Lindwurm, in an essay on typhoid in the hospital on the left bank of the Tsar, observes that it is generally believed that those who have been once affected by typhoid fever enjoy an immunity from subsequent attacks. He has, however, not unfrequently observed cases where the patients have been affected some years or even decades previously. Of course there can be no doubt that in such cases a new infection and a new introduction of typhoid into the system has taken place. Cases are also met with in which in the course of typhoid fever, after a longer or shorter remission of the symptoms, an exacerbation, or, more properly speaking, a relapse, takes place, with reappearance of symptoms that have already passed away (enlargement of the spleen, diarrhœa, roseola). In two cases sudden death occurred; and on examination, besides completely cicatrised typhoid ulcers, some perfectly fresh infiltration of the mesenteric glands and Peyer's patches was observed. As regards the etiology of typhoid, Prof. Lindwurm points out the great difference between typhoid and the acute exanthema in reference to infection in the hospital. Amongst 135 young persons lying in the same rooms with others suffering from typhoid fever, only one appeared to have become affected, and that was a doubtful case. The germs of typhoid are imbedded in the soil, and are more or less exposed or concealed with the rise and fall of the water level, but they may accumulate in the cracks of boards, interstices of stone floors, &c. From these localities they rise in the air and are inhaled or swallowed. That the dejections are carriers of the germs there can be no doubt, but whether the typhoid poison is reproduced and multiplies in the human economy is not satisfactorily made out. The rules for treatment are, to lower the fever, to give tone to the nervous system, and to strengthen the failing powers of the heart. To effect the first, the methodical application of cold baths is most important, whilst the other indications are met by the administration of quinine dissolved in hydrochloric acid, moderate doses of which should be given every two hours. A little white wine may be given to remove the taste. When the heart's action is very feeble, quinine should not be given in very large doses. As an excitant, Prof. Lindwurm recommends the subcutaneous injection of the officinal oleum camphoratum; from 15 to 30 grains of camphor can thus be introduced in the course of the day. (*Der Praktische Arzt*, No. 9, 1873.)

**Turpentine in Renal Affections.**—Dr. E. Martel, of St. Malo, records the following interesting cases in which turpentine proved of great service :—A man, aged forty-five, fell in the dark from a height of twelve or fourteen feet on some Dutch tiles. The left side was injured. The next day a large contusion was apparent on the left side, with a fracture of the corresponding iliac crest, the fracture separating a portion parallel to the border of the bone. From the time of the accident a considerable quantity of blood passed with the urine. On the succeeding days the ecchymosis became more and more pronounced, and the hæmaturia continued unchanged. There were no clots, but the blood was mingled with the urine, and a reddish black deposit took place ; there was no pain, nor any sign of lesion of the bladder. There were few or no febrile symptoms. The treatment was in the first instance external, but in consequence of the persistence of the hæmaturia, M. Martel administered turpentine in capsules internally, and found that in twenty-four hours the urine became limpid, and in ten days more the patient was able to travel. M. Martel has no doubt that in this case some lesion of the kidney occurred, and the action of the turpentine was in accordance with the old notions of its being a hæmostatic. In the second case recorded by Dr. Martel, a gentleman in good circumstances saw his father die from cancer of the stomach, and from apprehension that he should himself be a victim to it, became hypochondriacal, with some gastric derangement, indicated by slight pain and flatulence. The impairment of his wife's health necessitated relative continence. The spermatic discharge became red, and resembled the pulp of the gooseberry stained with blood, the staining probably taking place in the vesiculæ seminales. No gonorrhœa either past or present. M. Martel adopted a general tonic line of treatment, with cold baths, but, finding no improvement take place, sent him to Prof. Gubler, who prescribed turpentine, and under this treatment complete recovery occurred. (*Bulletin Général de Thérapeutique*, liv. 8, 1873.)

**Intra-uterine Polypus.**—Dr. M. Duncan, of Edinburgh, records a case of intra-uterine polypus occurring in an unmarried woman, aged 52, who had suffered for two years from discharge, at first watery and then bloody, and from lumbar pain. The uterus, which was examined by rupturing the hymen, was found to be enlarged and retroverted. The os uteri was enlarged by scissors, and the finger was passed into the uterine cavity, and a uterine polypus discovered  $1\frac{1}{2}$  inches in diameter. On attempting to remove it, it broke down, and much of it was washed away ; recovery took place. The tumour was care-

fully examined by Dr. Kronid Slavjansky, after maceration in bichromate of potash. It was found to be permeated by many canals of different sizes and shapes, between which lay common slightly fibrillar connective tissue. Higher powers showed that the canals were lined by a continuous stratum of short columnar epithelium. On the inner surface of the epithelial layer were spherical cells of a very transparent aspect. The capillaries were in some places very highly developed. In some parts were large clots of extravasated blood. The tumour was therefore of organoid character, and its structure was quite homologous with that of the mucous membrane of the uterus. He therefore applies to the tumour the name of *Adenoma polyposum hemorrhagicum uteri*. After some weeks, Dr. Duncan remarks in some further notes on the case, the pain and discharge again increased, and on examination the vagina was found to be filled with a soft tumour. The health was slowly failing. Dr. Duncan does not regard the disease as being cancerous in its nature. (*Obstetrical Journal*, Nov. 1873.)

**Connection of Inflammation of the Heart with Erysipelas of the Face.**—M. Jaccoud has observed that in a large number of cases inflammation of the heart, as endocarditis, pericarditis, or myocarditis, is associated with erysipelas of the face, and is disposed to regard the connection between it as close as that which exists between cardiac affections and acute articular rheumatism. In the majority of instances he finds the mitral valve affected, and more rarely the tricuspid; but he has never found the semilunar valves implicated. As a rule the cardiac disease is only temporary, and leaves no permanent defect behind it. The systolic bruit is most audible at the apex of the heart, and makes its appearance during the attack of erysipelas or a few hours before the occurrence of the latter. The bruit does not bear any direct relation in its loudness to the amount of febrile disturbance present. There is no articular rheumatism, pleurisy, or pneumonia accompanying the attack. (*Gazette Hebdomadaire*, 1873, No. 23.)

**Modern Treatment of Cholera in the Vienna School of Medicine.**—In the stage of diarrhoea, when the ordinary astringent means are insufficient, calomel and opium or bismuth are prescribed, and are found to act well. In this stage also hypodermic injections of morphia in the linea alba are efficacious. Thirst is best allayed by iced water or Seltzer water, sipped in small quantities at a time. Cramps are relieved by frictions with flannel and the application of extract of belladonna, opium, or hyoseyamus, or by the subcutaneous injection of morphia into the leg. For singultus, sinapisms, bladders of ice upon the

stomach, a twentieth of a grain of strychnine every second hour, a few drops of chloroform in water, or acetic ether with sugar or black coffee, are administered. In cerebral affections iced cloths are believed to be indicated. In the stadium algidum, when the pulse is very small and scarcely perceptible, when the skin is cold and blue, energetic stimulants are given, as moschus gr. iv. ; acetic ether, drachmas duas ; ol. menthæ, guttæ tres. In syncope and when reaction is required, more or less strong stimulants are indicated, such as camphor, musk, and ether. The two former may be administered hypodermically. The strength may be supported by Russian tea, and Malaga or Madeira wine. Great care is taken to prevent recurrence of the diarrhœa. When the urinary secretion is deficient, mild diuretics are prescribed, as juice of lemons, tincture of digitalis, Seltzer or soda water. In order to arouse the activity of the mucous membranes and of the kidneys, warm fomentations, frictions with ol. juniperi, unguent. digitalis, terebinth. venet., or with tincture of cantharides in the neighbourhood of the kidneys is indicated. During convalescence tonic remedies are given ; and if dyspepsia remain, nux vomica, rhubarb, and magnesia are prescribed. (*Allgem. Wiener Zeit.* 33, 1873.)

**Treatment of Snakebite and Hydrophobia.**—Dr. Donald Buller, Honorary Inspector-General of Hospitals, Bengal, states that he has had extended experience of snakebites. The sufferers, chiefly sipahis, were generally brought to him at night from five to twenty minutes after the accident happened. In one or two cases the only symptoms were a very weak circulation, coldness of the surface, nausea, and giddiness ; but in most of them these symptoms were accompanied by insensibility and difficulty of swallowing, and occasionally by violent spasms of the back and complete syncope. Dr. Buller's first care was always to apply two turns of a stout cord round the limb, immediately above the wound, so as to arrest the circulation through the superficial veins. A drachm of laudanum (100 drops) and an ounce of brandy (two table-spoonfuls) were then administered in two or three ounces of water, warm when procurable, with a little sugar and essence of peppermint, and this dose was repeated according to the urgency of the symptoms, till the returning circulation and heat of the surface indicated a favourable change—which was, he thinks, in all cases accelerated by making the patient walk about supported by two men. In severe cases the patient was in addition exposed to a large fire, and his throat, chest, and extremities were rubbed with laudanum, ammonia, and sulphuric ether. When the symptoms subsided the ligature was removed, and an ounce or two of Epsom salts administered. The proper treatment for *hydrophobia*,



he contends, is *prevention*, and should consist in washing the wound immediately with hot water, then blackening it with Indian ink, and washing this out again completely; then to apply lunar caustic over the whole of the wound, *to keep the wound open* for two months by the application of lunar caustic as often as the wound begins to heal, and to keep the system slightly under the action of mercury for that period. (*Pamphlet*, 1873.)

**Abortive Action of Quinine on Dogs.**—Dr. Rancillia states that it has often been remarked, both by Italian and French physicians, that the administration of quinine for the cure of intermittent fevers in pregnant women leads to abortion. Of late years, however, several American and Belgian physicians have maintained that the abortion is due not to the action of the quinine, but to that of the malarious poison itself. M. Rancillia's experience, however, which has been considerable as a veterinary surgeon in Caen, is quite in favour of the view that quinine is an abortifacient, since he has observed many cases in bitches where the uterine pains were feeble, and where ergot had been prescribed in vain, yet in which active pains were brought on by the administration of one and a half grains of quinine at short intervals. (*L'Union Médicale*, 1873, p. 800.)

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<sup>1</sup> Any of the foreign works may be procured on application to Messrs. Dulau, of Soho Square, W.C.; Williams and Norgate, of Henrietta Street, Covent Garden, W.C.; or Baillière, of King William Street, Charing Cross.



## Department of Public Health.

### PUBLIC ANALYSTS.

BY THOMAS STEVENSON, M.D.

MUCH attention has of late been directed to the proceedings of the public analysts appointed under the provisions of the Act of 1873 for the prevention of adulteration of food, drink, and drugs; and very diverse opinions are held respecting the merits and demerits of these functionaries. I fear that their merits are too little appreciated, their difficulties too little weighed, and their failures too highly magnified. By the public, and by their employers—the local authorities appointing them—they are considered too inactive; whilst the retail, and more especially the wholesale, traders regard them as public nuisances, if not worse. Again, chemists have complained that medical men have received appointments for which they are unfitted, and medical men have complained that chemists have been appointed to offices which require a special knowledge of medicine.

Seeing that the Act has been in force for little more than a year, and that perhaps no district analyst has been at work for more than nine or ten months, it may fairly be said that they have shown considerable activity; and I am persuaded that a large amount of good has already resulted to the community by the vigorous enforcement of the statute in a great many districts; whilst even in those places where the Act has hitherto remained a dead letter, a certain amount of benefit has accrued by the restrictions put upon the wholesale trade, and thus indirectly upon the petty local traders.

But it is more particularly to the difficulties experienced by the public analyst in the discharge of his official duties that I wish to direct the attention of the readers of the *Practitioner*, hoping by this means to enlist some sympathy for the analyst

rom the medical profession at large—a sympathy which, judging from the published articles in some professional journals, has been as yet little evinced. I feel convinced that the difficulties of the analyst will not be increased by being made known; and the free discussion of them cannot but tend towards their removal.

And first I wish to draw attention to the obscure wording and uncertain renderings of the statutes under which the public analyst is called to act; for as no steps can be taken to enforce the penalties imposed for adulteration without the analyst's certificate, it is most important that he should put no wrong construction upon the meaning of the clauses of the statute of 1873. What is an adulteration? What standards of purity are to be set up? Is the analyst bound to certify whether an impurity is or is not an adulteration? Is it necessary for him to determine how an impurity has gained access to an article of consumption? These, and many other questions of equal importance, constantly present themselves to the analyst, and require, at all events, some sort of solution at his hands. The crude manner in which the Act of 1872 has been drawn, and the consequently varied interpretations put upon its provisions by the legal profession and the justices, are the cause of much embarrassment to the analyst.

A typical instance of the uncertainties and dangers attending the enforcement of the Adulterations Acts has recently been furnished by the proceedings taken by the Westminster Board of Works for the alleged adulteration of *sp. éth. nitr.*; and by the side of this case we may place one more successfully carried out for the sophistication of effervescing citrate of magnesia. It is generally supposed that by the incorporation of the Pharmacy Act of 1868 with the Adulteration Act of 1872, any admixture of a drug is an adulteration. A sample of effervescing citrate of magnesia having been found by an analyst to consist of citric and tartaric acids with carbonate of soda and sugar, and to be destitute of any magnesian salt, a prosecution was successfully instituted and a fine imposed; although this was a case of *substitution* rather than *adulteration*. The Westminster Board, emboldened no doubt by the successful issue of these proceedings, prosecuted a certain person for selling adulterated spirit

of nitrous ether. It appeared, however, that the article had been sold as sweet spirit of nitre, and the summons was dismissed, the magistrate having a strong impression that sweet spirit of nitre is not a drug at all, and he being clearly of opinion that the mere fact that certain ingredients are omitted from the compound in the making up of the distillate cannot be called an adulteration. An ingenious distinction was also drawn between drugs, medicine, and medicinal compounds, and the magistrate seemed to be under the impression that a drug must of necessity be a dry substance, a restriction of meaning which, however interesting in a philological point of view, is one which will scarcely meet with general acceptance in the present day. The conduct of Dr. Dupré, the Westminster analyst, in the witness-box did him infinite credit. I hope that he will in no wise be daunted by the treatment he received, and that he will not be deterred by magisterial rebuke from expressing his opinion that certain practices are shameful adulterations. It is clearly the duty of an analyst to state what he believes to be an adulteration, whether his opinion to that effect be right or wrong; and we confess that we were surprised to find the presiding magistrate rebuking Dr. Dupré for the honest expression of his scientific opinion. The exemplary costs awarded to the defendant in this case will, alas, deter other local boards from taking any proceedings for the adulteration of drugs, whilst the sale of medicines and medicinal compounds will be subject to no adulteration law; and the shameful practice of selling citrate of iron and quinine containing 7 instead of 16 per cent. of quinine will go on unchecked. The public analyst does indeed labour under difficulties in analysing drugs and certifying to their adulteration.

Additional difficulties are sometimes thrown in the way of the analyst by his being required not to certify his *opinion* as to the adulteration of an article, but to state absolutely whether a given substance *is* or *is not* adulterated. It is the opinion of some of the legal advisers of benches of magistrates that the analyst is obliged to do this. The old-fashioned notion that a scientific witness ought to state facts, and his opinion founded upon facts, is thus rudely shaken, for the existence of an adulteration is a question of opinion, and not a matter of fact.

For instance, the question of the existence of the constituents of alum in bread is a question of fact; but these constituents having been found, a further question arises—is the bread adulterated or not? and this is entirely a matter of opinion, for the due determination of which many things must be taken into consideration, such as the quantity of alumina extracted, the relative proportion of sulphates in the loaf, the possibility of these as well as the alumina itself having had an accidental origin, and so forth. And the more careful and conscientious the analyst, the greater reserve will he exercise in certifying to the existence of an adulteration. In consequence (as I know to my cost) an analyst may be unfairly condemned for giving the benefit of a doubt to the side of the vendor of the suspected article, where an adulteration has been subsequently acknowledged.

The uncertainties attending the precise determination of the constituents of many articles, and the detection of certain adulterations, are also sources of great difficulty to the analyst. He is too often condemned for passing over the adulteration of butter with animal fats, of bread with rice and potatoes, &c., by those who are ignorant how in many cases the detection of these substances with the precision needful to depose in a court of justice to the existence of the adulteration is impossible. The vulgar notion is that if an analyst does not know everything, he ought to be able to do anything in the detection of adulterations—truly an excellent idea!

An unmerited stigma is often thrown upon the analyst, by those who ought to know better, by his being accused of making “half-crown” analyses. Anyone who has read the Adulteration Act must know that the half-crown or half-guinea fee paid for an analysis can in no case form the remuneration of the analyst for his work; the whole of the fee paid for the analysis must be paid over to the local authority, and this body assigns to the analyst a remuneration which is quite distinct from the fee paid to them for analysis by the purchaser of a sample.

The conflicting decisions of magistrates, and the extraordinary views of what are sometimes termed defence analysts, are sources of discouragement to the official analyst. One magistrate convicts for the skimming of milk, whilst another refuses even to grant a summons for the like offence; cocoa, if



mixed with sugar and starches, is deemed by some to be adulterated, whilst by other justices any sample is declared to be not fraudulently adulterated which contains the smallest quantity of cocoa, provided the manufacturer asserts that the article is improved by the admixture. Some non-official analysts go so far even as to say that all articles are unadulterated if they be "pure (or impure) as imported," as, for instance, tea weighted with 8 or 10 per cent. of mineral matter.

Notwithstanding the discouragements under which the official analysts are compelled to labour, I counsel them to persevere in their work, and to be undismayed by their difficulties. The Act of 1872 has proved on the whole an immense success, spite of great obvious failures. The Act has some clear defects, and as applied to drugs is perhaps inoperative. But the danger before us is that the statute should be too hastily altered. We have no fear that the Legislature will permit the reversal of the policy of the Act. Already the public is reaping the benefit of its repressive policy by being able to purchase a better and purer class of articles of food, even if in some cases at a slightly increased cost. Our bread is less alumed, our milk is less atrociously watered, and a vast number of trashy teas have disappeared from the shops. We hail with satisfaction the announcement recently made that there are now 10,000,000 pounds of fictitious, adulterated, and exhausted tea-leaves lying in bond, and that the importers dare not find a market for it in this country.

One great hardship which presses upon the analyst is this. The Act of 1872 does not require the analyst to make quantitative analyses, nor does any local authority give such remuneration as would justify an analyst in spending an enormous amount of time in making such analyses. Nevertheless, as a rule, magistrates require a statement of quantities before convicting.

Perhaps in the majority of cases a quantitative analysis is not needed: in the cases of coffee mixed with chicory and sold as pure, and of watered milk, the offence is sufficiently great to be worthy of a penalty, whether the admixture be 20 or 50 per cent. I cannot agree with those who would proportion the fine to the greatness of the admixture. To do so would be to enable the vendors of adulterated articles to compound their frauds in

some measure by a money payment. The act of adulteration is clearly not only a fraud upon the pocket, and often an attack upon the public health, but also an offence against morality.

Considering the large number of examinations made by public analysts, I think these gentlemen may be congratulated on the fewness of the instances in which their certificates have not stood the test of a second analysis.

### PUBLIC HEALTH INSPECTION.—CONTAGIOUS OPHTHALMIA IN POOR-LAW SCHOOLS.

MR. R. BRUDENELL CARTER, the Ophthalmic Surgeon to St. George's Hospital, in two articles lately published, has discussed in some detail the question of the prevalence of contagious ophthalmia in Poor-law schools. In one of these articles, which appears in the last volume of the St. George's Hospital Reports, he has dealt chiefly with the medical and surgical aspects of the subject; in the other, read before a conference of chairmen and vice-chairmen of boards of guardians, held under the auspices of the National Association for the Promotion of Social Science, Mr. S. S. Dickinson, M.P., in the chair, he dealt with the administrative aspect. It is well known that for many months past there has been an extraordinary prevalence of contagious ophthalmia in the metropolitan Poor-law schools, and the question which Mr. Carter has sought to elucidate is consequently of serious public interest. There is probably no disease of which the conditions of development and dissemination are more clearly understood or more completely under control than contagious ophthalmia. That this disease should in the present day attain large proportions among children subjected to the supervision which is supposed to exist in pauper schools is simply a scandal of the gravest character. A few years back contagious ophthalmia was one of the most mischievous of scourges of these schools, but under the influence of successive scandals, so great an improvement had been obtained in the sanitary condition and management, not only of the schools, but also of the children, that in late years it has commonly been



assumed that contagious ophthalmia had been practically banished from them. Indeed, in one of the oldest Poor-law schools of the metropolis, a school which had peculiarly suffered from the persistency and severity of the disease, it was the custom of the master, with justifiable pride, to point out to casual visitors the arrangements by which, under his own mastership, the disease had been eradicated in the school, and by which any chance introduction was altogether set aside. This school, we understand, has escaped from the recent prevalence of the malady, while, strange to say, several of the newly-erected schools have suffered severely. The uninitiated, knowing what had been done in some schools to extirpate the scourge, and with what success; knowing also that it was one of the mischiefs which, from the class of children received into these schools, had to be constantly guarded against, as the infection might at any moment be re-introduced and do more or less mischief; knowing, further, that the existing schools were under an elaborate system of supervision, local and central, and that each school had its medical officer, received the first news of an extensive prevalence of contagious ophthalmia in the schools with incredulity. This incredulity quickly passed into an amazed indignation as the facts became better known, and particularly when it was ascertained that in the greater number of the metropolitan Poor-law schools, new as well as old, cases of contagious ophthalmia were to be counted by scores, if not by hundreds. Whether the true history of this outbreak will ever see the light is more than we can affirm. To the present moment no official account of the outbreak has been made public, although it has been the subject of official inquiry; and in official quarters a guarded reticence is maintained on the subject, which is not without significance.

It will be a curious question, suggested but not referred to by Mr. Carter, whether the very freedom of Poor-law schools from contagious ophthalmia for several years immediately preceding the recent outbreak may not in some degree have contributed to it by lulling managers, medical officers, and Poor-law inspectors into a false security; in the same way as the declining severity of small-pox, under the extension of vaccination, largely begot carelessness of vaccination among those who had not experience, or

very little experience, of the disease, and so contributed to the development of the last great epidemic. It is a reasonable supposition that not a few medical officers and managers, who may never have had to contend with contagious ophthalmia in an epidemic form, or to become familiarised with it in a chronic form, had ceased to recognise it is a constantly present danger, and systematically to look for its first signs. In this point of view Mr. Carter's detailed and masterly description of the different forms of disease included under the general term "contagious ophthalmia," of their natural history, their consequences when unchecked, their treatment, and of the diseased state of the conjunctiva which forms the commonest preliminary condition of their development, is invaluable. We trust, indeed, that the paper published in the St. George's Hospital Reports, as also the paper read before the conference of chairmen of guardians (this being the complement of the former), will be published as a separate work. These papers should appear in a form which would make them accessible to all who have concern in the subject.

Now, taking it as a reasonable assumption that some medical officers and managers fell into the false security that we have suggested, we maintain that this is one of the dangers which it is particularly the duty of a central inspection to obviate. If inspection by the Central Authority does not provide for those necessary variations in the experience of changing officials, by bringing to their assistance the matured and continuous experience which a Government Department can secure, it is a sham of the worst description. Those of our readers who are familiar with the work which was done by the Medical Department then (but now unhappily no longer) of the Privy Council, will remember the immense energy displayed by the Department and its inspectors to rouse the profession, public vaccinators, and boards of guardians, as the local Vaccination Authorities, to a due sense of the growing danger arising from carelessness and neglect of vaccination. Had any like conception of the province of inspection governed the inspections of Poor-law schools, such a development of contagious ophthalmia as has lately occurred, having regard to the control which the Central Authority exercises over these schools, would have been impossible.

We have here anticipated in part, and from other considerations, the sum of Mr. Carter's observations. The prevalence of contagious ophthalmia in a school, or any other establishment, presupposes insufficient feeding, overcrowding, imperfect ventilation of schoolrooms and dormitories, defective washing arrangements, a generally unwholesome state of surroundings, and the absence of all proper means for separating the infected from the uninfected. Some or all of these conditions existed in the schools which suffered. *All* of them were found in the North Surrey schools at Anerley, according to the *Lancet Sanitary Commission*; and we have an official statement of some portion of the arrangements of that school, by Dr. Bridges, one of the Poor-law inspectors for the metropolitan district, a medical man. The following extract from a report of his, dated the 14th May, 1872, was quoted in the *Times*:—

“On the ground floor of the institution there are two wards allotted to boys. One of them is considered to be allotted to cases of ophthalmia, the other to cases of eruptions on the scalp. The former ward, however, contained a large number of cases other than eye-disease, and this on both occasions of my inspection. Some of the boys, from their own accounts (confirmed by that of the nurses), had contracted ophthalmia (as might have been foreseen) subsequently to their admission into the ward. This, however, is not all. Far too much latitude appears to me to be left to the nurses in the treatment of the children. On examining the head nurse, I found that there were two forms of lotion, described by her as ‘caustic lotion’ and ‘green lotion,’ used for the treatment of sore eyes. She admitted that she used in many cases her own discretion as to which of these two lotions should be used in each case. On speaking to the under-nurse of the boys’ ward, I found that she, too, was in the habit of using her own judgment as to medical treatment, especially with regard to the various kinds of ointment used for affections of the scalp, without even, in many cases, referring to the head nurse. The whole system is thoroughly objectionable, and it is, in my opinion, impossible to exculpate the medical officer. With 140 children in the sick wards, I found scarcely any written directions as to treatment. I am safe in saying that in more than 100 of the cases the medical officers’ directions for treatment, if given specially at all, were given verbally. Of course it would be simply impossible for any nurse to retain more than a tenth part of these directions in her memory.”

According to Mr. Carter, the report from which this extract is taken was not made on the motion of the inspector himself, or of his colleagues in metropolitan inspection, but it was called for by the Local Government Board as the results of special inspections directed to be made on account of squabbles among the managers and officers of the school, and an appeal

from them to the Central Authority calling attention to the prevalence of ophthalmia in the school. Here, then, we have a picture (not an uncommon one) of managers and officers at issue among themselves as to certain sanitary requirements of the school, and of a medical officer attempting to deal with a large amount of disease under conditions which render its proper treatment impracticable—a state of things which it might be presumed was pre-eminently one in which the interference of an inspector was called for. But it would appear that no efficient official cognisance of the circumstances in the school which had given rise to the disagreement among the managers and officers, or of the arrangements described by Dr. Bridges, was taken until asked for by the managers themselves, and not until the mischief which should have been prevented had been accomplished. For all purposes of prevention, and of assistance of the managers and officers in such prevention, the action of the Local Government Board and its inspectors seems to have been the completest sham. What was true of the Anerley school was true also, to a greater or less extent, of all the infected schools. “What does Dr. Bridges’ report prove?” asks Mr. Carter; and he answers:—

“First, undoubtedly, that Dr. Bridges, when employed as a medical inspector, is a careful and efficient one, and that his special knowledge enabled him at once to hit serious blots which a non-medical inspector would certainly have failed to notice. But it proves, also, that there had been no inspection into the sanitary condition of the establishment at any former period; for if there had been, the state of things described by Dr. Bridges could never have come to pass. Let us next inquire what was the result of the report. The managers of the schools at once endeavoured to introduce certain reforms; but they were left to do this very much by the light of their unaided judgment, instead of receiving the great help that a medical inspector might have afforded them. It is a significant indication of the previous state of the establishment that these reforms were not limited to matters of nursing and of infirmary management, but that they included improvements in the dietary of the children, a reconsideration of the number to be placed in each dormitory, an examination, ventilation, repair and sometimes complete re-arrangement of the drains, and structural alterations in the buildings occupied by infants. A few months after the report, and while reforms at Anerley were still said to be in progress, the Local Government Board positively relieved Dr. Bridges for a time from his duties in the metropolitan district, and sent him to distant parts of the kingdom upon a mission which was unconnected with his office, and which might have been equally well accomplished by any other physician; and in which he had as colleague an eminent hospital surgeon who is engaged in London practice. Nay, more, the Local Government Board does not appear to have called the attention of the non-medical inspectors to the



existence of ophthalmia and other serious evils at the schools ; for, about the time when the managers, having given each child a separate towel, allowed it to be spread out at his bed-head at night, as a sort of trap to catch contagious particles, one of the lay inspectors placed in the books a favourable report of the general state of the institution ; and this report was absolutely quoted by a manager as affording a sufficient answer to all criticism."

Mr. Carter did not refer to the sequel of his story, which, however, was freely referred to during the conference. Dr. Frederic J. Mouat, late Inspector-General of Prisons and Visiting Officer of Lunatic Asylums in the Bengal Presidency, was appointed to do Dr. Bridges' Poor-law work during the absence of that gentleman on the special commission referred to. Dr. Mouat appears to have been thoroughly alarmed by the state of things he found in the different schools, and as the representations of a man of his experience and position could not be shelved in the fashion of those of the Board's permanent inspectors, the Local Government Board allowed itself to be convinced that matters which were already a public scandal were of sufficient gravity to need active looking after. Dr. Mouat was requested to undertake the task of thoroughly inspecting the metropolitan Poor-law schools as to their sanitary state and the prevalence of contagious ophthalmia in them, and to advise the Local Government Board and the managers as to the measures required to improve the sanitary conditions of the schools and children. The recent circulars issued by the Local Government Board, as to the examination and when necessary quarantining of children before admission into schools, are no doubt a result of Dr. Mouat's inquiry. That it should have been necessary in the present year to issue instructions such as those contained in these circulars, referring as they do to the most rudimentary part of the sanitary management of Poor-law schools, is the strongest confirmation of the utterly unsound principles upon which the sanitary work of the Local Government Board is conducted.

How thoroughly an inspection of the kind conducted by Dr. Mouat was approved and appreciated by the metropolitan guardians, was warmly expressed by one of the representatives during the conference.

If the Local Government Board itself failed to appreciate a contingency so grave as that brought to its knowledge by Dr.

Bridges' report, it is not to be expected that its non-medical inspectors will display any higher appreciation of the sanitary matters which come under their observation. Mr. Carter argues that the want of comprehension on the part of the Poor-law Board of what constitute an efficient sanitary inspection, which led to the evils disclosed by the *Lancet Sanitary Commission* in 1866, still exists with the Board under its new name, and is producing now, as it did then, evils of like character, of which this outbreak of contagious ophthalmia is an example. He maintains that, consistently with all trustworthy experience, the inspection of workhouses and of Poor-law schools should be, as in the case of lunatic asylums, a conjoined legal and medical inspection. In support of this opinion he gives a letter from Dr. J. Crichton Browne, the Medical Superintendent of the West Riding Lunatic Asylum, too valuable not to be quoted here in full :—

“You are correct in saying that the sanitary state of our county pauper lunatic asylums contrast most favourably with that of the schools described, and is indeed maintained at the highest possible degree of excellence. Considering the weak and debilitated condition of most of the patients who are accommodated in these asylums, and the dirty and degraded habits of many of them, it is very remarkable that they are but rarely visited by epidemic or contagious diseases ; that these never attain any extensive diffusion in them, and that tubercular diseases are less frequent in them than in the general community, calculating the proportion upon persons above five years of age in each. Filled with the refuse of workhouses, with street sweepings, with aged, broken-down, and constitutionally-diseased persons, they yet enjoy an immunity from preventible disease, and maintain their death-rate at a point which is marvellously low when their peculiar circumstances are considered ; and this, too, although many of these asylums, the older ones, labour under every conceivable defect of site and structure. Now this excellent sanitary state of our pauper asylums I have no hesitation in ascribing to three co-efficients—firstly, to the constant and watchful supervision of their resident medical officers ; secondly, to the liberal administration of their governing bodies, the visiting magistrates, and to the immediate and respectful attention given by them to all medical recommendations ; and thirdly, to the vigilant and skilled inspection of the Commissioners in Lunacy. It is by the co-operation of these that the successful management of lunatic asylums is secured. Medical supervision would avail little, unless the magistrates were prepared to adopt the recommendations which that supervision is perpetually suggesting. Magisterial enlightenment would be powerless unless guided by scientific and technical knowledge, and both of these would speedily degenerate and ‘lose the name of action,’ if not stimulated and aided by central authority. As you are chiefly interested at present in ascertaining the part played by the Commissioners in Lunacy in contributing to the favourable hygienic conditions and general good management of asylums, I shall not say anything now as to the work done by the medical officers or magistrates, but shall confine myself to the value to be attached to official inspection, combined medical and legal. That



inspection, so combined, after a long experience, I venture to say, is essential to the results attained. Without it, all sorts of abuses would grow up—routine, negligence, and false economy would conduct us back to the worst days of asylum history. We should have mechanical restraint, incessant seclusion, cruelty, and wholesale slaughter by epidemics. Without the influence of the Commissioners, our asylums would, I am certain, speedily deteriorate, and without its medical element that influence would be nugatory. I do not wish to depreciate the value of the legal part of the Board of Lunacy. On the contrary, I attach the highest importance to it, and believe that by associating a lawyer with a doctor you get the best possible combination for inspectorial purposes over public institutions. Without the lawyer, the doctor is apt to be crotchety; but without the doctor the lawyer is altogether futile; for innumerable questions are constantly arising in asylum administration involving an acquaintance with cerebral pathology, with the natural history of disease, with the principles of science, and with sanitary details. When they visit an asylum, the Commissioners do not merely see and interrogate the patients with a view to ascertain their mental condition, and whether they are legally detained, but they inspect every room, corridor, lavatory, and closet, satisfying themselves that ventilation and warming are properly attended to, and that ample cubic space and floor area are allowed to each patient. They examine the clothing and bedding as to their quality and cleanliness; they see the meals served, and ascertain that the dietary is adequate in amount and is sufficiently varied; they inquire into the classification of the inmates, the means of employment, recreation, and exercise provided for them, the efficiency, humanity, and rates of remuneration of the nurses and attendants who wait upon them, the washing and bathing arrangements, the character of the water supply, and the system adopted for the disposal of sewerage. They also examine the case-books and post-mortem records, minutely and surgically investigating any accident or cases of sudden or violent death which may have occurred, and inquire into the general health of the community, requiring special information respecting any instance of epidemic or contagious disorder that may have happened. In these and a hundred other matters that command the attention of the Commissioners, but that it would weary you to enumerate, it is clear that the lawyer without the doctor would be entirely at the mercy of the local medical functionaries, and could not possibly exercise any salutary control over them. But it is not only during their periodical inspections that the Commissioners influence and direct the medical and hygienic arrangement of asylums, and so contribute to their success and healthiness. Every important event occurring in an asylum, the death of every inmate, with the cause thereof, and every outbreak of epidemic and contagious disease, are at once reported to them, and special inquiries are instituted by them whenever suspicious circumstances are present. Besides that, they regulate the selection of sites for new asylums, and the construction and arrangement of all buildings, enlargements, and alterations, and insist that these shall be carried out upon sound scientific principles. Altogether, they exert a most strict, judicious, and beneficial supervision over our asylums at every point. I am quite sure of this, that if a similar authority with similar powers were placed over our pauper schools, we should soon hear no more of contagious ophthalmia."

Mr. Carter rightly observed upon this letter, that "if a man of Dr. Crichton Browne's position and attainments feels that an

efficient medical inspection is a valuable aid to himself and asylums generally, how much more would his words apply to institutions that are placed in many respects in a less advantageous position? It is implied, of course, in the term efficient inspection that the recommendations of the inspectors are to be carried into effect." Mr. Carter added the following weighty words:—"And here I cannot forget that under the Public Health Act of 1872, many of the boards of guardians are the sanitary authorities for their respective unions. I cannot refrain from reminding them that the kind of inspection which was peculiar to the old Poor-law Board, which permitted the growth of the state of workhouse infirmaries described by the *Lancet Commission*, and which is, I believe, responsible for the conditions in which all the epidemics of contagious ophthalmia have had their origin, is the same kind which has been deliberately introduced by the Local Government Board to carry out the important and complex provisions of the Public Health Acts. I cannot refrain, as one necessarily somewhat conversant with sanitary matters, from entering my most emphatic and earnest protest against the employment of such inspection for such purposes, feeling certain, as I do, that it will not only be altogether futile as regards the objects to be attained, but that it cannot fail to be productive of very grave disasters."

We shall not weaken these words by comment.

## DWELLINGS OF THE POOR.

THIS complicated and important question has rarely been dealt with so comprehensively as in a Report of the Dwellings' Committee of the Charity Organisation Society. The report deals mainly with the question as it occurs in London, but the conclusions are applicable to all towns where like conditions of habitation obtain with those which the committee seeks to remedy. Passing under review the proved insufficiency of charitable agencies and commercial agencies alone, and of the Sanitary Laws as regards both nuisances and uninhabitable houses, to "check the tendency in large cities to the formation

of squalid and unhealthy quarters, where the poor are crowded together in unsuitable habitations"—although each of these agencies plays an essential part in promoting the end in view—the committee comes to the conclusion that the only agency which can house the poorer classes, with a due regard to their moral and physical welfare, and (an almost equally necessary provision) near their work—"is municipal government, acting with enlarged powers, and through the instrumentality of commercial enterprise, partly in its individual, but chiefly in its associated form."

The conditions which render philanthropic, semi-philanthropic, associated, and other commercial agencies, as things are, incapable of coping with the evil under consideration, are so clearly stated by the committee, that we cite its observations in detail:—

"The committee scarcely think it necessary to contend that if the work of repair and reconstruction is to be attempted in the interior of London on a comprehensive plan, the means can only be supplied on the commercial principle. No means can be shown of undertakings of such magnitude carried out from motives of disinterested benevolence; and if philanthropic agencies were employed side by side with commercial agencies, without accurate limitation as to the persons to be benefited, the free operations of capital would be embarrassed, and serious injury would be inflicted on the very classes for whom it is intended to provide. Philanthropic agency in building dwellings for the poor, means the supply of one of the chief necessities of life, viz. lodging, below its market value. Were such a practice to be extensively or indiscriminately sanctioned, not only would the profits of commercial investments be impaired, but the principle of self-dependence would be attacked, habits of self-indulgence would be encouraged, and even the wages of unskilled labour might be reduced. In the opinion of the committee, the greatest care should be taken by those who are entrusted with the administration of philanthropic foundations of this nature, that admission should be restricted to the very lowest order of self-supporting labourers, and to individuals in that order who would be likely without this form of assistance to become objects of charitable relief in some other form. Assuming, then, that the associated enterprise here referred to must be strictly founded on the commercial principle, and must have a free, undivided field of action, it still appears that this agency would be incompetent under present circumstances to surmount a momentous difficulty—the difficulty of obtaining sites. There is nothing in the physical conditions of London which should render the great work of reconstruction impracticable or unprofitable for associated capital. The ground is valuable, and in some parts thickly occupied by houses, but their arrangement is irregular, the dwellings separately considered are often of little value, and they are far from supplying the maximum accommodation which should be placed upon the area. It is certain that by systematic distribution, by economy of space, and greater elevation in the structures, one-half or more people might be lodged in a comfortable and wholesome manner, where the

present occupants are huddled together in dirt, discomfort, and disease. That the undertaking can be rendered fairly remunerative has been proved by the profits of existing companies. The principal, almost the only, obstacle to the prosecution of this great reform lies in the inability to purchase, and to purchase particularly for this specific object. So long as building associations are left to deal with corporate or private proprietors, directly and unaided, the work must be crippled and tardy. Powers of compulsory purchase can alone meet the difficulty, and as such powers will scarcely be bestowed for general purposes on private associations, it is indispensable that municipal government should step in as an intermediary between the builders of the future dwelling and the possessor of the existing one. By no other means can the field be cleared for great improvements, or associated enterprise be relieved from the fetters which now arrest its action and paralyse its immense powers of doing good" (pp. 11, 12.)

Now the compulsory power here contemplated is neither new nor untried in Great Britain. Compulsory powers of this kind have been granted to a greater or less extent, and have been for several years acted upon, in Liverpool, Glasgow, and Edinburgh. The experience of Glasgow is particularly to the point, and the committee had the advantage of receiving from the Lord Provost an explanation of the compulsory powers enjoyed by the Municipality of that city and of their administration to the present time. This explanation is thus stated:—

"The authorities of Glasgow, for some time previous to 1866, had been deeply impressed by the spectacle of social evils consequent on the overcrowding of the population in mean and ruinous dwellings. Many houses were without the requisite air and light. Many were mere dens of criminals and paupers. A resolution was adopted to obtain a Local Improvement Act, to deal with the increasing evil. It was procured in the year 1866, and was renewed in 1871. It contemplates a specific plan of comprehensive changes. It embodies provisions enabling the Lord Provost, Magistrates, and Council of the city, who are appointed trustees for carrying out the purposes of the Act, to take lands and houses compulsorily; to make new streets, to alter old ones; to purchase lands and houses by agreement, to erect new buildings, to dispose of land by lease or fee for building purposes, to erect and maintain houses specifically destined for the working classes, to borrow for these purposes a maximum sum of £1,250,000, and to charge the interest of the sum borrowed on the town rates, levying an annual rate for the purposes of the Act not exceeding sixpence in the pound. In conformity with the provisions of this Act upwards of £1,000,000 had been borrowed or received up to the month of June 1872, and upwards of £1,000,000 had been expended on the purchase of property, a portion of which had been profitably resold, while the greater part was provisionally held and let, but in an improved condition, and yielding £24,000 per annum. In the course of these operations seats of contagious fever had been extirpated, haunts of prostitution, crime, and intemperance had been broken up or restricted, and streets of new houses and shops of various kinds had been erected, but with due regard to the accommodation of the working classes. The building operations had been throughout conducted by private agencies, which came at once to the aid of the trustees, and



acted readily under certain prescribed instructions of sanitary construction. The Town Council had itself erected only two model (common) lodging-houses, which had been occupied at once, and had proved a very profitable investment. At the outset of the undertaking the Town Council had contemplated a rate of *sixpence* in the pound, and a loss on the whole transaction of £200,000; but the sixpence rate had only been requisite for a single year; it was kept at *fourpence* for two years, and had since been reduced to *threepence*, with a prospect of soon being fixed at *twopence*, while the capital loss was not now expected to exceed £50,000 at most. The process of demolition and reconstruction having been carried out with deliberation, no suffering had been inflicted on the poor, who were further protected by a provision in the Act which prohibited the ejection of more than 500 of the population at once, without a certificate from the sheriff that accommodation was obtainable in the neighbourhood. Without the powers of compulsory acquisition and compensation these great results could never have been obtained; but practically, almost the whole of the property had been secured by friendly negotiation" (pp. 17-19.)

With this indisputable evidence before it, the committee of the Charity Organisation Society recommends that, without neglecting "minor remedies," the attention and action of the Council "should be in a great measure concentrated on one principal object, viz., that the most extensive and effectual powers of purchase, demolition, and direct or delegated reconstruction, should be vested in the chief existing municipal authorities of London—the Corporation and the Metropolitan Board of Works—in order that they may follow the example set by the Municipalities of Glasgow, Edinburgh, and other provincial cities which have been armed by Local Acts with such prerogatives, and that the Corporation and Metropolitan Board should be urged to use their powers, when obtained, in a bold and comprehensive manner, but with special regard to the interests of the poorer members of the community."

We fully concur in the recommendation of the committee, but for "minor remedies," the phrase it uses, we would substitute the words "other remedies." For in reality none of the agencies employed in dealing with the difficult question which the committee discusses can rightly be regarded as minor, even by comparison with one another. Each has its place, and each is requisite to the particular end. Take for example the supervision of tenemented houses which may be carried out under the Sanitary Act 1866 (sec. 35). No conceivable plan of building can do away with the necessity of its exercise if the tendency to the formation of squalid and unwholesome quarters in towns



is to be restrained. We not long ago had the opportunity of inspecting certain model dwellings erected on the most improved plans and with the most philanthropic intentions, in the midst of a most degraded quarter of a great manufacturing town. Philanthropy had become wearied of the persistent struggle required to regulate the debased habits of the occupiers; the Municipality had not put the building under such supervision as was needed; and there, before the stone of which the block of building was constructed had lost its freshness, the different tenements, the appurtenances, and the surroundings had fallen into a state of squalor and unwholesomeness which rivalled the miserable dwellings in the vicinity. Again, we have recently seen in the environs of the metropolis, streets of admirably planned and commodious new cottages, erected for a single family, systematically let in occupation to two families, and undergoing a process of deterioration, under the double occupation, which will quickly bring the buildings to a state of deplorable foulness and unwholesomeness, and will require the most energetic action of the sanitary authority to remedy. Let us avoid the terms "minor" and "major" in dealing with the different agencies we already possess, or which we hope to possess, for the improvement of the condition of the dwellings of the poor. But at the same time we agree with the committee of the Sanitary Organisation Society, that it is proper to concentrate our energies in order to obtain such additional powers to this end as have been proved to be necessary.

#### THE AUGMENTED MORTALITY OF THE TEN YEARS 1861-70.

It is very probable that not a few persons whose attention may have been arrested by the Return of Deaths for the decennial period 1861-70, the general results of which were briefly stated in the last number of the *Practitioner*, will have been puzzled to account for the increased mortality there shown as compared with the previous decennial period, 1851-60. It is true that the increase of the rate of mortality during the

last decennial period has been slight (2,242 as compared with 2,217 per 100,000 population), and that for practical purposes the mortality may be regarded as having been stationary ; but nevertheless there is an increase, and an explanation may well be asked how it has come to pass that the large amount of sanitary work done during the period under consideration should apparently have been productive of no result upon the general mortality of the kingdom, and that the large sums of money expended on such work have seemingly yielded no return in life and health. This explanation is not difficult. The result does not in the least make doubtful either the value of or the necessity for sanitary work. It simply means that such work has been as yet but partially carried out, and that it has only sufficed to keep the general mortality for some years virtually stationary. This conclusion may excite surprise among those who have little familiarity with the details of the subject. It is no doubt difficult to believe that all the energetic work on sanitary matters of the last twenty-five years should have had so meagre a result. Yet such we believe to be the fact. That work has indeed been chiefly limited to large town populations, and even to a few only of these ; and until the passing of the Public Health Act 1872, it was not possible (such being the imperfect state of the law) to carry out in any great degree like work in small towns and rural districts. It is not too much to say that in the great mass of villages, the majority of small towns, and not a few large towns, no real improvement in their sanitary condition has taken place during the past quarter of a century ; and in some towns and villages the sanitary condition has even steadily deteriorated. Nay, during this period, from the defective state of the law, it may be said broadly, that in almost all the new centres of population which have been created in connection with mines and manufactures, the worst sanitary evils of the older centres have been reproduced ; and these newest places, which might have and ought to have set examples of sanitary order and propriety, have rivalled the older and least-cared-for places in all that was objectionable and unwholesome.

An instructive illustration of the little progress which thorough sanitary work has to the present time made in this country,

may be obtained from the official inquiry which was carried out by Dr. Buchanan, for the Medical Department of the Privy Council in 1865 and 1866, "On the results which have hitherto been gained in various parts of England by works and regulations designed to promote the Public Health." (*Ninth Report of the Medical Officer of the Privy Council.*) This inquiry related to twenty-five towns with an aggregate population of more than 600,000 persons; and in several of these towns sanitary improvements had been too recently carried out to exercise any marked effect upon the mortality. The small number of places included in Dr. Buchanan's inquiry, it is to be inferred, arose from the paucity of towns which, having regard to the state of sanitary work in them, admitted of such an inquiry. Indeed, Dr. Buchanan says that "the towns were chosen, after consultation with the Local Government Act Office, as being places where structural works had been most thoroughly done and had been longest in operation, and were not chosen for any previously ascertained improvement in the health. They are believed to represent a considerable majority of the towns in England that are available for the purposes of the inquiry." And Mr. Simon, commenting on Dr. Buchanan's inquiry, observes: "It was essential that the inquiry should not be prematurely made; and it was not till two years ago that I felt myself entitled to hope that the undertaking could be free from such reproach."

If we insist upon this point of the comparatively small extent, in respect to numbers of towns and villages, to which sanitary works have as yet been carried out in England, it is because we are anxious to impress upon our readers the necessity for correction of the common misapprehension which exists upon the subject. No doubt that the sanitary work which has been done here contrasts very favourably in amount with what may have been done in other countries; but it is very far indeed from being as considerable and as general as is needed. We do not in the least degree desire to under-rate the work which has been done. We know well its magnitude; and we also know the enormous difficulties which have had to be overcome in carrying out sanitary works, and which still impede their progress. But it is important that we should not form a false estimate of the amount of what has been done, and so anticipate

from it results as respects the general health of the kingdom which this amount is not calculated to yield.

It is a perfectly proper assumption that the effects of sanitary work should be shown in a diminution of the general rate of mortality; that, in fact, the general rate of mortality may be taken as a gauge of the wholesomeness or unwholesomeness of a place. The evidence supplied by Dr. Buchanan from his inquiry is conclusive on this point, while at the same time it furnishes the fullest confirmation of the value of sanitary works. We quote the results obtained by Dr. Buchanan:—

Town.	Population, 1861.	Change in death-rate (after execution of works) per 10,000.
Cardiff ... ..	32,954	From 332 to 226
Newport (Mon.) ... ..	24,756	„ 318 „ 216
Croydon ... ..	30,229	„ 237 „ 190
Macclesfield ... ..	27,475	„ 298 „ 237
Salisbury ... ..	9,030	„ 275 „ 219
Merthyr ... ..	52,778	„ 332 „ 262
Brynmawr ... ..	6,334	„ 273 „ 332
Ely ... ..	7,847	„ 239 „ 205½
Banbury ... ..	10,238	„ 234 „ 205
Ashby-de-la-Zouch ... ..	3,840	„ 216 „ 202½
Carlisle ... ..	39,417	„ 284 „ 261
Warwick ... ..	10,570	„ 227 „ 210
Dover ... ..	23,108	„ 225½ „ 209
Stratford-on-Avon ... ..	6,823	„ 217 „ 202
Morpeth ... ..	4,490	„ 262 „ 247
Alnwick ... ..	6,494	„ 262 „ 247
Leicester ... ..	68,056	„ 264 „ 252
Cheltenham ... ..	39,693	„ 194 „ 185
Rugby ... ..	7,818	„ 191 „ 186
Bristol with Clifton ... ..	160,714	„ 245½ „ 242
Penrith ... ..	7,189	„ 253½ „ 250
Worthing ... ..	5,805	„ 155 „ 153
Penzance ... ..	9,414	„ 221 „ 222
Chelmsford ... ..	8,664	„ 196½ „ 215

On the results of his inquiry as to reduction of total mortality, Dr. Buchanan remarks:—

“In the great majority of the twenty-five towns there has been distinct reduction of the total death-rate. In nine of them, having an aggregate population of 204,641, this reduction has amounted to about a fifth part of the previous rate of death. In eleven other towns the fall in the death-rate has been less considerable. In three or four towns (including one that has

scarcely finished its works) the rate has been stationary, and one shows an increase in the rate of mortality since the period when the works designed for its improvement were executed.

"Some of the reasons for the position of towns in the above list readily occur. Thus, at the head of the list with chief reduction of mortality are towns where the previous death-rate was most excessive, as in Cardiff, Newport, Merthyr, where it had exceeded 300 in the 10,000 yearly; and, secondly, towns where, along with sanitary improvements, notable change in the social or industrial conditions of the population has taken place; of this the chief examples are Croydon and Macclesfield. At the other end of the list appear towns that cannot yet have got the full fruit of their sanitary work, as Bristol and Penzance; or that had previously a death-rate close upon the necessary annual rate, as Cheltenham, Rugby, Worthing, and Chelmsford. These considerations require to be kept plainly in view if a judgment of the relative value of sanitary measures in various towns is to be obtained."

Of Chelmsford, Dr. Buchanan writes:—"While very much has been done for the improvement of the town, there is to each sort of work some drawback from complete efficiency."

These details (and very much besides in Dr. Buchanan's report), while demonstrating the value of works carried out for the promotion of public health, serve to confirm the interpretation that we have put upon the augmented mortality of the last decennial period. This augmented mortality in no wise militates against the efficacy of sanitary works, but is simply an indication that such works have not yet become so common in the kingdom as to reduce the general death-rate.

ERRATA.—Two misprints occurred in the article on "Quixotic Pathology" in the last number of the *Practitioner*, the author having been unable to correct the press. Last line but one, p. 464, for "severe and fatal miasmas," read "severe and fatal invasions." Second paragraph, seventh line, p. 467, for "so far as is known, ten cases occurred," read "so far as is known, no cases occurred."



# THE PRACTITIONER.

FEBRUARY, 1874.

## Original Communications.

### ON THE TREATMENT OF TYPHOID FEVER WITH COLD BATHS.<sup>1</sup>

BY PROFESSOR BEHIER.

I DO not intend to give you, to-day, a complete and didactic exposition of the different therapeutic indications which typhoid fever offers, and the measures of treatment that correspond to these indications. My chief object is to call your attention to a method of treatment, too little diffused among us, and which you have seen me apply to the patients in the Salle St. Antoine, whose disease I have just been studying with you. I wish to speak of the methodic use of *cold plunge baths*. I hasten to state that I do not partake of the exclusive enthusiasm which certain authors profess for this mode of treatment, nor do I recommend its universal and systematic employment. But I believe that we have here a real and efficacious progress, which I have made a point of exhibiting to you, and which I have it at heart to propagate and to popularise to the extent of my power.

The necessity of combating the symptom *fever* has for a long time past become a truth which is almost a childish truism.

<sup>1</sup> From a clinical lecture delivered at the Hôtel-Dieu. Translated from the *Bulletin de Thérapeutique*.

The times are gone by in which, according to the old Hippocratic idea, the febrile movement was considered as an effort of nature destined to subdue or to eliminate the morbid principle, a salutary effort which the physician ought rather to favour than to oppose. We now know that this disorderly action is always dangerous and often fatal: dangerous by the exaggeration of internal oxidations and the rapid consumption which accompany the febrile movement: dangerous above all by the modifications of the blood which are produced by these excessive oxidations, which pour into the common medium organic débris which are often very abundant, and which are not sufficiently eliminated. But the elevation of temperature is the index and the result, if not the cause (or one of the causes) of this febrile state. We may hope, then, that the intensity and the consequences of the fever will be diminished by means which will reduce the temperature of the body. But if there be a malady in which the efforts of treatment should especially be addressed to the element *fever*, surely it is in the most obstinate and long-continued of our pyrexias—in typhoid fever.

It is about twelve years since I was one of the first to introduce into our country the employment of a valuable therapeutic agent in febrile affections. I speak of *alcohol*. This is not the time or place to speak of the internal action of this remedy. Does it diminish the combustion of the tissue by directly opposing itself to that process, just as, when in a certain degree of concentration, it hinders the continuation of alcoholic fermentation? I believe this, but it is of little consequence to us just now. An invariable fact is, that in sufficient dose it lowers the central temperature both in the normal and in the febrile states; it is therefore an antipyretic remedy. It is in this character, and not as a tonic, that you see me administer alcohol, in drinkable shape, to our febrile patients, often in considerable doses, under the euphemistic designation of *potion de Todd*. Observe carefully, however, that these doses are divided. Thus, you will prescribe 80 to 200 grammes (3 to 7 ounces) of brandy in 8 ounces of water with a drachm and a half of tincture of cannella and an ounce of syrup. You will give a tablespoonful of this every two hours, diluted with

three or four times as much water, if the patient finds the mixture too strong. This remedy, I repeat, gives good results; but I have no hesitation in saying that we are in possession of a much more energetic and habitually efficacious antipyretic remedy in the rational employment of cold water, of which I now wish to speak to you.

It was the English physician, James Currie, who first had recourse in a regular and scientific manner to the external employment of cold water in the pyrexias. The patient, in full fever, was placed in an empty tub, and several cans of water were poured over his head and trunk from a certain height. The method of Currie consisted, then, in the use of *cold affusions* more or less frequently repeated. The object which he proposed to himself was not to cause an abstraction of heat and a diminution of the internal temperature, but to obtain, by these smart and brief affusions with cold water, a consecutive excitement of the circulatory functions, or what is called *reaction*.<sup>1</sup>

There are a certain number of cases in which cold and sharp affusions are formally indicated: this is especially the case in adynamic conditions with profound intellectual torpor and a tendency to depression of the respiratory and circulatory functions. The affusions, by lashing, as it were, the activity of the nervous centres, dissipate, better than any other means, these conditions, which are allied rather to a defect of innervation than to febrile elevation of temperature. This is not the object which one attempts to gain in employing cold water according to the method which we are studying. In that method we propose, on the contrary, to abstract a certain quantity of the heat the augmentation of which is crushing the patient. To this end we employ cool water, more or less smartly applied. A first degree consists in cold washings of the entire body, repeated four or five times a day: you saw me adopt this plan with the young girl in bed No. 8, Salle St. Antoine. In her, the affection of the bowel presented itself with an *ensemble* of moderate symptoms, and, on the whole, was but little formidable; above all, the fever kept at a reassuring level; the medium temperature was 39° C. (102°·2 F.) in the evening, and 38°·5 C. (101°·5 F.) in the morning. There was thus no indication for an energetic anti-

<sup>1</sup> This scarcely does Currie justice.—Ed. *Pract.*

pyretic treatment, and we confined ourselves to giving 3 oz. of brandy, and using the cold spongings. We thus managed to keep the fever to its moderate type, and on the twentieth day the girl entered definitely the apyretic stage. I do not doubt that the cold spongings assisted this rapid and fortunate issue.

The man who restored to honour the treatment of the pyrexias, and especially of typhoid fever, by cold water, is a practitioner of Stettin, Ernest Brand. In 1861 he published the results of his treatment in a book entitled "*Hydrotherapy of Typhus.*" This treatment constituted a true method in the rigorous and dogmatic acceptance of the word. In a recent publication M. Franz Glénard has expounded the system of the Stettin physician with talent, and has related observations on thirteen patients treated at the Hôpital de la Croix-Rousse rigorously and in the spirit of the method. Liebermeister, in speaking of Brand's book, while acknowledging the decisive impulse which it has given to the therapeutics of febrile affections, regrets to find in it a too ardent and enthusiastic proselytism. We might pass the same criticism on the memoir, otherwise very interesting, by M. Glénard. We may say, in passing, that one can easily understand the enthusiasm of M. Glénard. The method, as you will see, offers great advantages; but, besides this, M. Brand has a right to the respectful gratitude of everyone in our country of France.<sup>1</sup> . . . .

This treatment of Brand's consists in the regular and systematic administration of cool baths (20° C. or 68° F.), with or without the affusion of cold water upon the head. The regular duration of each bath is fifteen minutes, even when shivering occurs quite at the commencement.

It requires a certain energy on the part of the physician to apply this method, considering the pitiable condition which the patient presents in some cases. On coming out of the bath the patient is enveloped in a woollen wrapper, and immediately covered with clothes.

On the average it is necessary to give at least eight baths in the twenty-four hours (a bath every three hours, night as well as day). According to Brand it is an error of the gravest kind

<sup>1</sup> A few expressions of strong and painful feeling, growing out of the events of the war, are here omitted.—*Ed. Pract.*

to wait till the temperature reaches a serious elevation; he says that we should prevent the rise of heat, and not wait to combat it. He denies that there is any efficacy in "those hybrid methods which give one or two baths a day, and which are nothing but a *cruelty* to the patient."

We see by these quotations that Brand is inspired with all the intolerances of a systematiser. In fact, the treatment by baths, one after the other, is, in his mind, not only directed against the symptom fever, but also against the essence of the morbid process; in a word, it is, what the author calls it, a *specific* treatment. It is evident that a treatment thus rigorously and mathematically formulated must rest upon a theory; that of Brand is the following. In agreement, so far, with recent mycological researches, he attributes the typhoid process to an internal fermentation, azymosis. But, says he, if we mix a solution of malt with a proper quantity of beer-yeast at a temperature of 16° C. (59° F.), we get a tumultuous fermentation producing alcohol after a determinate period of three days. If, then, we cool the mixture, which has risen to 35° C. (95° F.), below 16° C., the fermentation will cease; on the contrary, it will reappear if the temperature rises again. Brand, in view of these observations, supposes that the typhoid ferment, also, cannot develop and act except at a certain temperature, and hydrotherapy renders it powerless and arrests its production by incessant cooling of the blood.

I need not point out to you how much of the arbitrary there is in this theory; but it was important to direct attention to it, for it shows well what is the essential indication which Brand thinks that he fulfils by his treatment. What he seeks to obtain by this pitiless series of immersions is not only the lowering of temperature; his views are higher than this; he wishes to reach the very source of the evil; he aims at arresting in this manner the morbid fermentation which is the point of departure of the disease.

Hence the rigid inflexibility in his prescriptions; one must be incessantly on one's guard and watch the ferment for fear it should begin to work again and cause us to lose all the benefit of the treatment at once.

Such is the method by which hydrotherapy has made its



reappearance in the treatment of typhoid fever: it is the same which M. Glénard recommends. But in these latter years our conceptions of fever are singularly modified, and the water treatment of pyrexias—especially of typhoid fever—has shared in that progress. It is this last phase into which the question that occupies us has entered.

The febrile elevation of temperature can only be due to two causes; either a diminution of the waste of heat, production remaining the same; or else an increased production of heat, the sources of waste remaining unaltered. The first of these views is maintained by Traube, the second by the majority of observers, who support it especially by reference to the increase, during the febrile act, of the excretion of urea and carbonic acid, which is an indisputable proof of a positive augmentation of organic combustion. Quite recently, in a memoir containing laborious researches on the inner significance of febrile phenomena, Senator<sup>1</sup> arrived at a mixed conclusion, which unites the two views; according to him, the febrile heat is due at once to increase of combustion and to retention of caloric.

I propound these theories to you only in order better to prepare your mind for appreciating the treatment. Whatever may be the fate of the doctrines which have just been laid before you, the therapeutic indication in these cases appears to be a simple one; to increase the loss of heat by plunging the patient into a cooler medium with which he will tend to equalise his temperature. But affairs are far from proceeding with such simplicity; man, like all warm-blooded animals, enjoys the privilege of striving against the sources of heat-waste, in virtue of what is called the *heat-regulating power*.

If we place a healthy person in a cold bath, the loss of heat will be moderated by a double mechanism. On the one hand the spasmodic contraction of the peripheral vessels, by diminishing the cutaneous circulation, repels the mass of the blood towards the deeper cavities, where it is better shielded from loss of heat by radiation. At the same time, the production of heat itself is notably increased, as it has been proved by the quantity of carbonic acid which is exhaled. It is thus not easy to reduce the internal temperature of a healthy person, con-

<sup>1</sup> Neue Untersuchungen über den fieberhaften Process. Berlin, 1873.

siderably, by a cold bath. We only produce a slight lowering, which is quickly compensated, and more than that, from the period known as that of *reaction*. It is only when the bath has been excessively prolonged or of a very low temperature that the cooling presents a certain intensity and duration, and it is then not free from danger.

As Liebermeister has pointed out, the conditions of regulation of temperature are the same in the fevered as in the healthy subject; and we may, like him, regard fever as a transitory but more or less enduring condition, during which the organism tends to maintain its equilibrium of temperature at a higher degree than the normal, at  $39^{\circ}\text{C}$ . ( $102^{\circ}\cdot2\text{ F}$ .) instead of  $37^{\circ}\text{C}$ . ( $98^{\circ}\cdot6\text{ F}$ .) The means by which the fevered organism lowers its temperature are the same as those employed by the healthy body; the resistance, however, is less energetic and less efficacious; in other terms, it is much more easy to lower the temperature by a degree or two in a fevered, than in a healthy man.

It is this, in fact, which favours the antipyretic action of hydrotherapy, and it is for want of taking heed of this capital difference between the normal and the febrile states that various authors have contended, on *à priori* grounds, against the employment of cold water in the treatment of pyrexias.

We see, then, that if cold water moderates fevers, it is not, perhaps, by moderating combustions (for it is proved, on the contrary, that it tends rather to increase them), but by hastening the departure, the waste, of caloric. Its action is thus diametrically opposed to that of alcohol, quinine, and other agents which directly attack and moderate the febrile combustion; its rôle is that of spoliation, not of economy of waste.

In an absolute point of view, hydrotherapy, in whatever form, does not constitute an anti-febrile remedy in the rigorous sense of the word; it does not restrain combustion, it only masks and attenuates to some extent the consequences of combustion, especially the elevation of the temperature of the blood.

Fever does, in fact, betray its action on the economy by two results which are distinct, though closely allied; the rapid consumption of the tissues on the one hand, and the elevation of

temperature on the other. Each of these conditions has its own dangers, and consequently affords precise indications. In the slow, chronic fevers (*hectics*, in a word), the danger consists less in the elevation of the temperature than in the gradual and progressive deperishing of the organism; it has therefore never entered the mind of any physician to employ cold water in these cases, and thereby to render more active the very wasting which threatens the patient. On the contrary, that which constitutes the immediate and direct danger in pyrexias when the fever is more acute, so to speak, is not so much the exaggerated histolysis as the very fact of the febrile increase of heat; the delirium, the convulsions, the drowsiness, the tendency to syncope or asphyxia, the whole *ensemble* of ataxic and adynamic phenomena, is due, we can hardly permit ourselves to doubt, to the pernicious action of a superheated blood upon the nervous centres. In a position such as this, our duty before all things is to cool the blood by the rapid extraction of heat from it; an indication which hydrotherapy alone is capable of fulfilling.

As I have now endeavoured to throw some light for you upon this question, it remains for me to describe to you the mode of treatment which I employed for our patients, and the way in which I believe it is best to arrange the cold baths in the treatment of typhoid fever. Here are the cases:—

I.—A young girl of 19 years, lying in No. 9, Salle St. Antoine. On admission (Nov. 1) she presented all the symptoms of grave typhoid fever. She was then in the sixth day of the disease; the morning temperature was  $40^{\circ} 2$  C. ( $104^{\circ} 3$  F.); that of the day,<sup>1</sup>  $40^{\circ} 6$  C. ( $105^{\circ} 1$  F.) The pulse was 120 to 130. The adynamia was more distinct; the patient was deaf. The gaze was fixed, stupefied, the features sombre and concentrated; she answered questions drily, brusquely, and incoherently. There were *subsultus tendinum* and fibrillary muscular tremors. The tongue and the teeth were foul, the belly moderately tympanitic, gurgling in the iliac fossa. No rose-coloured spots.

In the first days of her stay in our wards she was submitted to the treatment which we habitually apply in cases of this nature (cool spongings, application of many [24] dry cups to the trunk and limbs, *potion de Todd* 100 grammes).

But we did not find any sensible amendment, and on Nov. 8 (fourteenth day of the disease) the weakness was more and more pronounced; she lay on her back, in extreme prostration, with continual sub-delirium. The pulse was small and contracted, the breathing short, hurried, and facial. We observed a

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<sup>1</sup> Mid-day?—Ed. *Pract.*

marked relative dulness at the bases of both lungs, and abundance of fine sub-crepitant râles (hypostasis).

In presence of such a condition I thought of some more energetic measures; and I prescribed the use of cold baths. On the evening of the 9th the patient was placed in a bath of water at 20° C. (68° F.) She was kept in for about twelve minutes, when she began to shiver; she was then immediately taken out, rubbed briskly with a warm blanket, and replaced in bed.

Next day (the fifteenth) we found the patient in a veritable state of *collapse*. The temperature, which on the previous evening had been 40°·6 C. (105°·1 F.), was now 35°·8 C. (96°·4 F.); the pulse was small, hardly perceptible, 140 a minute. Nevertheless we gave a bath of thirteen minutes' duration. Half an hour after the bath, the patient being warmer again, the axillary temperature was 38°·4 C. instead of 35°·8 (a difference of nearly 5° F.) The pulse was also improved; it was firmer and slower (120).

From the date of this change that marked the beginning of the treatment, the latter was continued without any incident; it consisted of three baths daily of 20° C. (68° F.) temperature, and fifteen minutes' duration. After the 11th of Nov. the axillary temperature never exceeded 38°·2 C. (100°·8 F.); intelligence was completely restored, the sordes on the teeth disappeared, the tongue was moist, with only a thin covering of mucus. The numerous fine sub-crepitant râles that were heard before the treatment were replaced by fewer and coarser sounds; the hypostasis had almost entirely disappeared.

On the 17th we stopped the baths. Temperature normal, tongue moist and clean, appetite fairly good, intelligence perfect, convalescence frankly established. The duration of the disease had been twenty-four days. [Here follows a chart of the pulse and temperature which shows the striking change produced by the baths, but which we are obliged to omit.—*Ed. Pract.*]

II.—Marie C—, aged 19, No. 25, Salle de Antoine, admitted Nov. 2, 1873. She is a robust nurse, who has lived in Paris for six months, ill for the last four days. Tongue white, with red edges. Diarrhœa, gurgling in the right iliac fossa, numerous rose spots, deafness. Countenance stupid; *subsultus tendinum*, agitated delirium, absolute mutism. The first eight days of her stay in hospital she was treated with simple cold spongings, and *potion de Todd* 100 grammes; the mean temperature was 40° C. (104° F.) in the evening, and 39° C. (102°·2 F.) in the morning. On Nov. 8 (eleventh day of disease) there was stiffness of the neck, deglutition was impossible, and there were very distinct cerebro-spinal phenomena. We prescribed cold baths of 20° C. (65°·5 F.), given in the same way as to the former patient, three times in every twenty-four hours. From that day forward the temperature fell and remained always at a moderate level, only once over 39° C. (102°·2 F.) Intelligence and speech returned gradually; on the 20th (twenty-fourth day of the disease) the baths were stopped, the temperature was normal, and we began to feed the patient.

Assuredly we should not be in a position, on the mere authority of two cases, to pronounce definitely on the value of a treatment. Nevertheless, I determined to place these facts before you, because they are instructive in themselves; and for the rest, to establish their value, we can turn to the already very numerous documents which we meet with, on this subject, in medical literature.



In the mode of administering the baths I did not confine myself blindly to the (I had almost said) tyrannical orders of Brand. Each time that we gave a bath we did so because we thought it to be rendered necessary not by the exigencies of a programme laid down beforehand, but by some disquieting symptom—fever with temperature at or over  $102^{\circ}2$  F., delirium, collapse, &c. For the rest, the majority of the physicians who have adopted Brand's treatment—Bartels, Jürgendsen, Liebermeister, C. Schutzenberger, &c.—have not followed his empiric formula, and it is only very exceptionally that they have practised those frequent immersions which the Stettin physician recommends.

In a recent and interesting memoir, Wunderlich the younger describes the method in which the water treatment of typhoid fever is applied by his father in the hospital at Leipzig. There they do not give baths unless the morning temperature is at or over  $39^{\circ}$  C. ( $102^{\circ}2$  F.), and that of the evening at or over  $40^{\circ}$  C. ( $104^{\circ}$  F.) The baths are at  $18^{\circ}$  or  $20^{\circ}$  C. ( $64^{\circ}4$  or  $68^{\circ}$  F.); they last from fifteen to twenty minutes. If cerebral symptoms are early declared, Wunderlich employs baths even though the temperature be below  $102^{\circ}2$  F. An intense bronchitis or pneumonia is for him a primary and formal indication for hydrotherapeutic intervention. In our No. 9, we saw that a genuine hypostatic pneumonia existed, which disappeared very rapidly after the first immersions. Without doubt, the action of the cold stimulates the reflex activity of the pulmonary vessels, as it revives the cutaneous functions, and thus dissipates pain or congestions.

Brand allows of no contra-indication, unless it be that of intestinal perforation. Wunderlich is less absolute, and without refusing the benefits of hydrotherapy to patients who have attendant diarrhoea, albuminuria, heart-mischief, or pregnancy, he stipulates for a less energetic treatment, and the use of warmer and less prolonged baths. It is in these cases, I think, that we may usefully adopt the method of Ziemssen, which consists in placing the patient in a warm bath, the temperature of which is gradually lowered by adding cold water to it.

As a formal contra-indication, besides hæmorrhage and intestinal perforation, Wunderlich mentions collapse. As to the



last, I cannot share this view, and I believe, on the contrary, that the plunge bath, and cold affusions, are one of the best agents that we possess for dissipating that tendency to paralysis of the vasculo-respiratory centres which is the primary cause of collapse. Our patient in No. 9 is a convincing example of this. On the morning of the fifteenth day she was threatened directly with collapse, the nose and extremities were cold, the axillary temperature was at  $96^{\circ}8$  F., the pulse was thread-like and scarcely to be felt. Half an hour after the cold bath the patient had become warmer again, the pulse and temperature had risen, and all traces of collapse had disappeared.

In grave cases, of the pulmonary or intense cerebral form, Brand, Liebermeister, and Wunderlich recommend the application of iced compresses to the head and chest in the intervals of the baths.

Quite lately, some authors have endeavoured to perfect the machinery of refrigerant medication by proposing certain innovations which, to my thinking, are more ingenious than practical. Thus Senator, starting from the true observation that cold, by contracting the cutaneous vessels and anæmiating the skin, hinders the loss of heat, proposes the simultaneous employment of cold baths and of rubefacients to the skin. He says that he has had good results from applying numerous mustard-plasters to the surface of the chest and abdomen before the bath, plasters of such a kind that they can be left on when the patient is in the water. Lastly, of late a physician named Kemperdick has, in a case of typhoid fever, replaced the cold baths by injections of cold water into the rectum, by means of a catheter with a double current introduced a long way, and declares that in this way he succeeded in affecting the general temperature of the patient. It is doubtful whether such a practice offers sufficient advantages to compensate for the inconveniences which belong to it.

What are, definitely speaking, the advantages of this treatment, which have gained it so many fervent adepts, and which cause it, in certain countries—Germany for instance—to be employed to the exclusion of almost every other remedy? This is the final point for us to elucidate. A simple glance of the eye, cast upon our thermometric curves, will spare you, so to

speaking, all commentary. You see that by means of a few baths administered in each twenty-four hours, we succeed in beating down and mastering a febrile movement which everything seemed to show would be violent and lasting. In satisfying this essential indication, the lowering of temperature, hydrotherapy weakens at the same stroke almost the whole group of the symptoms of typhoid pyrexia. The nervous centres are the first and the most happily influenced; the delirium departs, the intellectual torpor is banished, the patient comes back to himself and interests himself in what surrounds him; at the same time the carphology, the *subsultus tendinum*, and all the other indices of profound nervous perturbation disappear. The respiratory centre also participates in this remarkable modification; the inspirations become deeper, slower, more powerful; hæmatisation is effected more completely, and the bronchi, resuming their contractility, expel the secretion which obstructs them. The skin resumes its tone and its suppleness; the blood, propelled by the heart in stronger and more regular waves, circulates more actively, and revives the stagnant secretions. Even the digestive canal presents a notable improvement, the tongue cleans and becomes moist, thirst subsides, and tympany diminishes.

Such is the detail of each important symptom: now let us address ourselves to that high criterion of every therapeutic method, the study of the mortality; we shall demonstrate an encouraging result. Brand declares that of 171 patients treated by his plan up to 1868, 170 were cured. According to him a fatal result *cannot* happen if the method be carried out rigorously. But it is proper to remark that Brand's figures have been made up mostly from practice in the towns, which is much more favourable than hospital practice. M. Glénard saw at Stettin, in 1870 and 1871, 89 patients all cured; and of 12 patients at Lyons, treated by M. Glénard himself, all were cured by the method of Brand.

Other authors, without obtaining such absolute success, have nevertheless proved the very remarkable efficacy of this method of treatment. We find, in the already quoted memoir of V. Wunderlich, the following statistical results:—

In the Leipzig clinic there were treated *without* hydrotherapy,

from 1851 to 1867 (16 years), 1,178 cases of typhoid fever, with 213 deaths; *i.e.* a mortality of 18·1 per cent. From 1868 to 1872 there were treated, *by cold baths*, 251 cases with 18 deaths; *i.e.* a mortality of only 7·2 per cent.

Here, moreover, are other statistics from the same author, which are still more impressive :—

	Mortality with treatment without baths.	Mortality with treatment with baths.
Jürgendsen . . . . .	15·4 per cent.	3·1
Ziemssen and Immermann . . .	30·2 „	7·5
Liebermeister and Hagenbach . .	26·2 „	9·7
Riegel . . . . .	20 „	4·3
Stöhr . . . . .	20·7 „	6·6

These figures appear to test the treatment well.

There remains one sole objection—the following: the cold, by forcing the blood towards the internal parts, may favour the production of intestinal hæmorrhages.

The statistics of the Leipzig clinic bear out this idea, in appearance at least. In fact, of 251 typhoid patients treated with cold water, 18 had intestinal hæmorrhage, or 7·1 per cent.; whereas, according to the tables of Louis, Griesinger, Ragaine, &c., with ordinary treatment hæmorrhage does not occur in more than 3 to 4 per cent. of the cases.

Wunderlich thinks that the Leipzig statistics are derived from too small a number of patients (251) to be decisive, and believes that we ought to consider the influence of series. He does not lay the blame of the greater frequency of hæmorrhage upon the baths, for these bleedings, in the 18 cases in which they were observed, were not produced immediately after immersion, but some hours, often half a day, afterwards, when the centripetal repulsion of the blood no longer existed. Finally, which is a fact of the highest importance, these cases of hæmorrhage were not serious, and the whole number ended in cure.

According to Wunderlich it would be precisely the employment of the baths which so singularly diminished the gravity of the complication in these cases. The patient, thanks to the treatment, is placed in such conditions that he bears these hæmorrhages much better, and they become, so to speak, innocuous. Thus, says he, if intestinal hæmorrhages are made

more frequent, their gravity is much diminished, by cold baths. But before accepting this dictum of Wunderlich, we must remember that I had occasion to point out to you that bleeding from the bowel in typhoid fever has not always the gravity which is usually ascribed to it, and that Graves, Trousseau, and myself had observed many cases in which hæmorrhage terminated favourably. I shall not return to this point, which I have already studied in detail with you, but it is well to remember it when we are appreciating the dictum of Wunderlich.

The cases which you have witnessed in our wards, and in which cold water has been employed, have doubtless appeared to you significant in themselves; they were, as you observed, grave forms of disease, which gave us the most lively anxiety: their termination, so rapid and so fortunate, is evidence not only of the harmlessness, but of the power, of the treatment which we thought it our duty to employ. It will be objected, that the number of my cases is so small; agreed, but the statistics of others fill up this hiatus. There is always room for dispute respecting an indication which is based on the physiological action of a remedy; but figures are not so easily to be criticised. A therapeutic method which can show a mortality relatively so small ought to be universally applied; it is not so much a question of progress as of humanity. I shall therefore think myself very happy, gentlemen, if my voice find an echo among you, and if you consent to share my convictions and help to spread them. You have seen: say and repeat everywhere what you have seen; that will be the best and most honest propaganda that you can help me to make.

[The Editor feels that he need scarcely apologise for printing this most excellent lecture, instead of an "original" paper properly so called.]

## THE BROMIDE QUESTION.

BY DR. J. WARBURTON BEGBIE.

EDINBURGH, Jan. 15, 1874.

DEAR DR. ANSTIE,—I have read with much interest in the *Practitioner* for this month, your translation of the paper by Professor Binz, of Bonn, on “the therapeutic employment of bromide of potassium,” and your own account of “the English stand-point respecting the value of bromide of potassium.”

Were it not that you have inadvertently fallen into error in ascribing to me the authorship of the article on bromide of potassium which appeared in the *Edinburgh Medical Journal* for December 1866, I should not have troubled you with this letter. My appreciation of the value of bromide of potassium as a remedy in various diseases is, however, so high, that I am jealous of any testimony which has been borne to its therapeutic actions failing to exert the influence which such testimony justly possesses. Permit me therefore to state that the article in question was written by my father, the late Dr. Begbie, whose patient investigation of the virtues possessed by bromide of potassium and strong recommendation of its use did much to secure for it the confidence of the profession in this part of the country, and largely contributed to its general popularity.

Having made this correction, I am encouraged by the perusal of what you have written to add my testimony to the value of bromide of potassium in the treatment of various diseases. This I shall do in a categorical manner, in something of the same way as you have recently done.

1. *Epilepsy*.—In this disease my experience of bromide of potassium entirely confirms the statements of Dr. Russell



Reynolds. I have repeatedly seen cures, in the strictest sense of the word, result from its employment, after the failure moreover, in many cases, of other remedies. Not only have severe fits ceased to return under its use, but the general health of patients, and more especially their mental condition, which had seriously suffered, have been completely restored.

Years have elapsed in certain instances since the occurrence of a fit, and individuals who had, owing to the frequency and severity of their attacks, been rendered incapacitated for their employment, have been enabled to resume their occupations and continue them without interruption.

Some patients have been benefited but not cured. The fits in such have been rendered less frequent in their occurrence, and less severe, but have not been entirely removed. Still, in these cases the bromide of potassium has been truly the *summum remedium*. No regulation of diet, no peculiar stringency of regimen, no other remedy than bromide of potassium—and many remedies have been tried—has exerted the like beneficial influence. Accordingly, it has in such cases been continued for the purpose, as you and Dr. Hughlings Jackson have observed, of reducing the frequency and severity of the fits. Some cases of epilepsy have in my hands been in no respect benefited by the bromide of potassium. A few have apparently been aggravated. I have not been able to satisfy myself of the reason for this varied but exceptional experience. It is, however, a common experience in the use of other admirable remedies. *Nullum medicamentum est idem omnibus*.

Looking back upon my experience of epilepsy, I feel inclined to remark that, were I deprived of the bromide of potassium, I should conclude that my best hope of being useful to the sufferers from this last disease had been taken away. I cordially embrace the aphoristic deliverance of the authority whom you quote in the concluding sentence of your paper: "It has changed the whole prognostic significance of epileptic attacks."

2. *Insomnia*.—In the procuring of sleep, bromide of potassium may be said to fall far short of opium, chloral, henbane, and other narcotics; and yet in many cases of insomnia it is superior as a remedy to the whole of these. Its innocency is

in the first place to be set against their potency, not unmixed as that potency is with injury or even danger.

In the sleeplessness which precedes mental shock, as is occasioned by long-continued mental strain or by worry, the bromide of potassium in full dose is oftentimes singularly efficacious, not only procuring much-needed sleep, but tranquillising the whole nervous system, and rendering the individual, otherwise quite unfit, capable of mental exertion.

I have repeatedly prescribed the remedy with the happiest results in cases of insomnia accompanied by general restlessness and incapacity for exertion, consequent upon long-sustained mental effort with anxiety in professional men, and on prolonged devotion to business in persons following different kinds of mercantile pursuits, in whom rest, change of air and scene, the most careful attention to diet and regimen, including treatment in hydropathic establishments, and the use of other drugs, had entirely failed to produce any good result. I do not affirm that the bromide of potassium always succeeds, or that it has always succeeded in such cases. I entirely concur in your observation that the insomnia of aged persons is apt to be aggravated by the bromide of potassium, although I have not found it to be always so, as your experience appears to have been. In one case of an old lady the remedy certainly did harm. She, however, had notable calcareous degeneration of the arterial tissues; and from my observations in her case and in other old persons, I have been led to surmise that the condition in question interferes with the physiological action of the salt, and with its therapeutic action likewise. Bromide of potassium is believed to contract the minute vessels, and if degeneration of their walls exists to a marked degree, in failing to produce this effect it is possible that the presence of the salt in the blood may excite cerebral disturbance in place of quelling it. Whether this theory be correct or not, I have for a considerable time avoided the use of bromide of potassium in old people whose vessels were evidently the seat of general atheromatous degeneration, but have prescribed it in the insomnia of the aged when this morbid condition of the vascular system was not conspicuous.

A further and most important use of bromide of potassium is as an adjunct to chloral. I have found 20 grains of the

former greatly increase the efficiency of a like dose of the latter.

The insomnia of delirium tremens is often overcome by large and frequently repeated doses of bromide of potassium, and so also is the sleepless excitement of puerperal mania. In these maladies the combination of bromide of potassium and chloral is chiefly to be recommended.

I have had occasion to verify the important observation of Dr. Begbie, that the craving for alcoholic stimulants which is so distressing a feature of dysomania, is to a certain extent, even in some bad cases, and to a much greater degree in the milder, restrained by bromide of potassium.

3. *Spasmodic Diseases*.—In controlling habitual cramps of the lower extremities, I have found no remedy so useful as bromide of potassium; and undoubtedly the very distressing cramps of the formidable Asiatic cholera were found during the last prevalence of that disease to be subject to its influence.

In spasmodic asthma I have had a considerable experience of bromide of potassium, and have here but to rank its virtues very highly. In a review of the late lamented Dr. Hyde Salter's excellent work on asthma, undertaken at the request of my friend Dr. Sanders, then editor (1869) of the *Edinburgh Medical Journal*, I took occasion to express a favourable opinion of this salt as a remedy in asthma, and, at the same time, surprise that it had not even been named by Dr. Salter.

Perhaps in the treatment of asthma no remedy has appeared to me so useful as the iodide of potassium, but in my experience bromide of potassium has effected a cure when the iodide has failed. The union of these two salts, and their combination with arsenic, has been still more efficacious.

4. *In the incontinence of urine of young children*, bromide of potassium has answered when even belladonna had not succeeded, and these two remedies are probably the most available in this often troublesome disorder.

A less experience of its use in the following diseases has led me to the conclusion that in each of them the bromide of potassium is a remedy well deserving trial:—Hysteria, more especially its convulsive forms (in these Sir Charles Locock had reliable proof of its value), gonorrhœa, and certain non-malignant en-

largements of the liver and spleen, the former more especially when connected with the too free use of alcoholic drinks.

I forbear from mentioning the diseases in which the use of bromide of potassium has been followed by results either negative or wholly unsatisfactory. Let me, however, state that I have grave doubts of its being, in the strict sense of the term, a febrifuge. In relieving the restlessness and insomnia of the febrile state, it unquestionably does good: but such therapeutic action does not entitle bromide of potassium to rank as an antipyretic.

I agree to the fullest extent in your judgment of the value of such articles as that of Professor Binz, in forcing us to scrutinise our grounds of belief in the action of remedies with additional rigour. Although I regard Professor Binz as essentially wrong, it will be my duty, after reading his paper, to reconsider the position I have been led to assume.

Believe me, dear Dr. Anstie, yours sincerely,

J. W. BEGBIE.

## THE BROMIDE OF POTASSIUM IN THE TREATMENT OF GONORRHŒA.

BY JOHN W. BLIGH, M.D., MONTREAL,

*Late House Surgeon, Kidderminster Infirmary.*

SOME three or four years ago, when engaged in investigating the therapeutic action of the bromide of potassium, I was so strongly impressed with its apparent adaptability to the treatment of gonorrhœa, that I determined to employ it in that disease whenever a suitable case came under my care. The success which followed its first administration was sufficiently gratifying to induce me to test its efficacy more fully as further opportunities offered. This I have done on various occasions since that time, with results which I can conscientiously characterise as uniformly satisfactory. Unfortunately, my opportunities have not been so numerous as might be desired, on account of the comparatively limited number of patients treated for this disease at the institution with which I was formerly connected. For this reason, as well as from the difficulty which must always exist in accurately determining the *post hoc* from the *propter hoc*, in a disease which *per se* tends towards recovery, I cannot speak so authoritatively as I might otherwise wish. I will therefore restrict myself to a plain statement of its apparent *modus operandi*, of the *complications* in which I have found it most useful, and the mode of procedure adopted in its *administration*.

ITS MODUS OPERANDI.—As the result of careful and repeated experiments, bromide of potassium is said to have the power, amongst its various other properties, of—



1. Diminishing the secreting functions of all mucous surfaces.
2. Acting as a direct sedative to all the nerves which supply the mucous membranes.
3. Increasing the flow of urine.
4. Acting as a direct sedative to the organs of generation.

It will be found to act beneficially in all the four foregoing ways. By acting as a direct sedative to the organs of generation, it prevents an increased flow of blood to the parts, thus preventing engorgement, with subsequent increased inflammation, so often followed by exudation and its train of ugly after-symptoms. Thus, through its power of diminishing the secreting functions of all mucous surfaces and acting as a direct sedative to all the nerves which supply them, it limits the gonorrhœal discharge and relieves the pain in micturition, by rendering the urethra less susceptible of irritation. In common with other remedies which augment the secretion of urine, the bromide of potassium renders this fluid less stimulating, by increasing its flow. It will thus be found serviceable throughout the whole course of the disease, but more especially in the inflammatory stage, in which so many other remedies are debarred.

This salt seems to have the power of diminishing the irritability of mucous membranes, not only when taken internally, but also when applied topically, for, according to Dr. Ringer, some writers aver "that merely brushing the pharynx and soft palate with a solution is sufficient to quell the irritability of these parts, so as to enable a laryngoscopic examination to be made with ease." Assuming such to be the case, it may naturally be inferred that a similar effect would be produced by its topical application to the inflamed urethra. This, I believe, will be found to be the case, for which reason an injection is recommended, as will be seen when its mode of administration comes to be considered.

COMPLICATIONS.—It may be used with advantage in some of the complications to which gonorrhœa gives rise, especially in that distressing and frequently troublesome one, chordee. For by reason of its power as a direct sedative of the organs of generation, it subdues the sexual appetite and prevents any tendency to erection, which, as is well known—when inflammation has extended to the corpus spongiosum

urethræ, and the cells are glued together by the effusion of coagulable lymph—is sure to produce this painful effect.

It will also be found useful in the third stage, when the disease has degenerated into a gleet.

MODE OF ADMINISTRATION.—As soon as a patient complains of gonorrhœa, the bromide of potassium should be immediately commenced, and continued throughout the duration of the disease. As it is said to increase the acidity of the urine, a condition not at all desirable, some alkaline bicarbonate should be combined with it to counteract this tendency. The following formula has been found useful:—

℞. Potassæ bicarbonatis gr. 60  
Potassii bromidi gr. 90—120  
Tincturæ hyoscyami f. ʒss  
Aquæ camphoræ f. ʒ vss  
Mis. fiat mistura.

One-sixth part of this mixture to be taken three times a day, and once during the night, should the patient happen to be awake.

Care should be taken not to administer a dose whilst a meal is in process of digestion in the stomach, as it may, by neutralising the gastric juice, interfere with the conversion of the food into chyme.

If the disease is in the first stage, an injection of the salt is ordered and recommended to be used as frequently as opportunities allow. The following is the usual form and strength in which I employ it:—

℞. Potassii bromidi gr. 120  
Glycerini f. ʒss  
Aquæ distillatæ f. ʒ vss  
Mis. fiat injectio.

One syringe-ful to be used every four hours.

When the discharge has assumed the form of gleet, a similar injection, associated or not as may be thought advisable with some astringent, will be found useful. In addition, I am accustomed to administer during this latter stage from fifteen to twenty grain doses, three times a day, combined with fifteen minims of the tincture of the perchloride of iron, and dissolved in some suitable menstruum.

[N.B.—I see no reason why an injection of the bromide should be contra-indicated in the inflammatory stage, but, on the contrary, consider its anæsthetic properties (when applied locally) as likely to be of much service. I am, however, unable to speak from experience, never having made use of it in this stage.]

When there is any disposition to painful erections or chordee, a draught containing about half a drachm in an ounce of camphor mixture, administered at bedtime, will be found to allay this tendency almost to a certainty. In this complication its effect seems magical, and has only to be tried to be recognised as a boon of inestimable value.

There are certain accessories which should not be neglected in this, any more than in any other plan of treatment. The bowels should be carefully regulated, the proper diet prescribed, and a total abstinence from beer and other stimulants insisted on. Rest should be enjoined, and over-exertion strictly avoided. The testicles should be supported by a suspensory bandage, and the genitals bathed from time to time, especially before retiring to rest. The flow of urine may be increased by the free use of diluents, as lins ed tea, barley water, &c.

My object in being thus precise in detailing its mode of administration has been to endeavour to induce others, who enjoy a far wider field for experiment than mine, to make use of the drug; when I feel confident its value as an additional agent towards the cure of a loathsome disease will be appreciated, and my views herein expressed verified. In the whole range of the Pharmacopœia there is no drug which in my opinion *primâ facie* promises such happy results in the treatment of gonorrhœa. For, as has already been pointed out, not only do we find it to have the power of diminishing the secretions and assuaging the pain of all mucous membranes, but also to produce a *special* sedative effect upon the organs of generation. Undue administration has been said to give rise to debility, but exhibited in the doses here recommended, all fear of this kind may be laid aside. Indeed, from considerable experience in its administration in large doses, continued over a lengthened period for the relief of epilepsy, I am inclined to think that its ill effects have been greatly exaggerated.

## SINGULAR CASE OF CHLORAL-POISONING.

BY DR. ANSTIE.

THE following case, which has lately come under my observation, presents some features which have not, I believe, been yet described. It is a remarkable instance of the continued use of chloral in enormous quantities, the final dose, which brought matters to a crisis, being quite unparalleled in medical literature, so far as I am aware. And the symptoms which were produced are in several respects unlike anything which has been recorded as a result of the abuse of chloral.

In December last I was summoned to attend a medical man, whom I found in bed and in an alarming condition. He was pale, haggard, with suffused eyes; he tossed about incessantly and complained of agonising pains in all his limbs. He told me that he had scarcely slept at all for a week, except on the day previous to my visit, and concluded his preliminary statement by informing me that he traced the whole of his miseries to the abuse of chloral.

His story in detail, as I afterwards obtained it, is substantially as follows. About February 1st, 1873, he began the use of hydrate of chloral, in ʒss doses, in order to procure sleep at a time when he was harassed by great anxieties. About this time he was attacked with modified small-pox, followed by scattered abscesses; and this led to his continuing the chloral into the month of April, sometimes increasing the usual dose, and sometimes taking a second if he awoke very soon. About two months from the time of his first employing the drug, he noticed the first unpleasant effect; his eyes became "inflamed

and weakened, and there was burning lachrymation." He left off the habitual use of chloral, but took it, perhaps, one night in three or four, when wakeful. The usual dose, at this time, he thinks, was about ʒj, from April to August.

In August, while travelling, he recommenced the habitual use of chloral, and then first took it *during the day*, as a calmative, once, twice, or thrice daily. About the first of September he noticed that the use of any alcoholic stimulant, even claret in very small quantities, would flush his whole face, suffuse his eyes, and bring on almost at once a severe headache, which was almost always at the back of the head, "about the cerebellum:" this peculiarity continued up to the time when I saw him. During the whole interval he had continued to take chloral with the exception of one week for which he abandoned it: he then did not sleep, for five nights, until 4 A.M., when he would doze off, and waken moist with perspiration. It was about the beginning of December when he first realised with accuracy what quantities of chloral he had been taking, as he had previously been accustomed to take it pretty much by guess-work. He found that he had been taking *over half an ounce in the twenty-four hours*, some days more than others. His appetite was not much affected; the bowels were regular, rather relaxed in warm weather. Coming from Nice to Paris he experienced very evil effects from the change of temperature: at the latter place he got severe general pains, particularly about the joints; these grew worse in the moist and cold weather at Paris and London, to which place he next came. Chloral did not relieve these pains at all except when it put him to sleep. They were peculiar in character, there was no tenderness anywhere, and no aggravation from movements.

On the day before I first saw him he made a mistake with his dose. Having been accustomed to make a dilute solution from a strong one which he kept ready, by some blunder he took a dose of the latter instead of the former. He slept during the day, but got up and went to dinner in the coffee-room of his hotel at 7.30 P.M. He had no sleep during the following night, and the pains came on with frightful severity. I was sent for the next day, and saw him about 6 P.M. Careful inquiry made it apparent that the dose which he had taken on the previous



day was *over one ounce of hydrate of chloral*. It was only by degrees, however, that I came to credit this fact, for at first his manner led me to suspect that he did not know what he was saying.

I made a careful study of the patient's state when I saw him. He was a somewhat slight, middle-aged man, but well made and fairly muscular. His eyes were suffused and half covered by the drooping eyelids; there was a wistful, haggard expression in the face; he expressed himself in a peculiar broken speech which at first gave me quite a wrong suspicion. I thought he was partially drunk, and that the story about habitual abuse of chloral was a blind to conceal habits of chronic toping; but a very brief inquiry showed that there was not the slightest ground for this notion.

The treatment which I adopted had but little effect, except the negative part of it. I at once decidedly forbade him ever to take a single grain of chloral again under any pretext whatever, and obtained the promise of his wife that she would see this order carried out; but I must do him the justice to say that he made no hesitation about obeying me, although the ordeal was very painful. I prescribed chloride of ammonium to relieve his pains, and extract of *cannabis indica* to give him sleep. On the following morning I found him easier, though he had very little actual sleep. In the course of another twenty-four hours we had to give up the chloride, as it was producing gastric irritation, but he continued the *cannabis* for some days. It should be mentioned, here, that my patient was very peculiar in respect to ordinary hypnotics. Opium, he said, never agreed with him; it always aggravated the effects of the chloral, and besides this caused peculiar tetanic spasms: a solitary dose of opium which I prescribed for him (being somewhat incredulous of his statement) produced this effect in a marked manner. He told me that it was accompanied by a sense of loss of control over the muscles. He had tried *hyoseyamus* (in extract) on various occasions, but it never had the slightest effect in any dose. Bromide of potassium in one-drachm doses never produced sleep, but it somewhat relieved some symptoms which I had omitted to mention, namely, constant *timnitus aurium* and a "singing sensation" which he declared seemed to come from the back of his neck, or, as he said,

from the medulla oblongata: these feelings had been upon him for about a month. To belladonna and to aconite he believes he is sensitive in the ordinary way.

My patient went on, with one device or another, parrying the temptation to recur to chloral; and in about a fortnight he might be considered fairly over the serious part of his difficulties. He was still somewhat pale, though not muscularly weak; and I may remark that except for a short time after the enormous dose of chloral which he had taken, when there undoubtedly was not merely leg-weakness, but some want of co-ordinative power, he does not appear to have suffered any diminution of muscular force. I had put him on an acid and iron tonic, on account of a rather distinct anæmia; but as this somewhat disagreed, he was put on bromide of potash and calumba. He was recovering his power of natural sleep very fast, and the pains had almost entirely disappeared. He declared, with every appearance of sincerity, that he would never touch chloral again; and I believe he will not. There was one more symptom about him which, together with the remedy which it suggested, I must finally mention. When he had somewhat recovered he asked me about a condition which had been very pronounced during the acute poisoning, and persisted for some time afterwards—namely, a singular dryness of the skin. This appeared to me to be due to an exaggerated tonicity of the cutaneous vessels, and I suggested that aconite might very probably give much relief to his insomnia when the latter was accompanied by this condition of the cutaneous vessels. My own experience most amply confirms what has been said about the power of aconite to relax the cutaneous vessels; it is probably the most active drug in this direction which we possess; and I have on many occasions seen insomnia which was accompanied by dry harsh skin yield to the administration of aconite in repeated small doses, *coup sur coup*. The advantage of fractionising the dose is, that you get a good massive quantity into the system without causing the fainting or sickness which it might have produced if taken all at once.

The case above related appears to me not merely very curious and interesting, but suggestive of new precautions in diagnosis and treatment. Its pathological novelty (independent of the

enormous doses taken) consists in the fact that the prominent feature was pains in the neighbourhood of the joints. Now these pains very strictly resemble the analogous sufferings which are (somewhat rarely) produced by chronic alcoholism: they do not run in the course of the nerves like neuralgia, nor are they exactly in the joints, like articular rheumatism; they seem to *encircle* the limb, the finger, &c., immediately above or below a joint. Similar pains may, as I have pointed out in a previous article in this journal,<sup>1</sup> be produced by prolonged abuse of chloroform inhalation; and it is very interesting to perceive that chloral acts similarly to chloroform; thus confirming, *pro tanto*, the idea that it is decomposed in the blood with the effect of setting chloroform free there.

Another feature in the case, though not exactly novel, is probably unfamiliar to many practitioners, viz., the curious effect of chronic chloralism in producing intolerance of alcohol. It is mentioned by Ludwig Kern, in a paper which I lately translated for the *Practitioner* from the *Deutsch. Arch.*: it is there stated to depend upon a weakened condition of the vaso-motor nerves, especially those of the cranial and facial regions. It is a curious fact, which I have observed in more than one patient addicted to chloral, that this condition of the blood-vessels of the head may be attended by a precisely opposite state—namely spasm—of the vessels of the extremities: it was markedly so in the subject of this paper. The exact relation of chloral to the vaso-motor system has not yet, I think, been satisfactorily described; no doubt Professor Burdon-Sanderson could tell us much about it, as animals are now so frequently chloralised for the purpose of performing experiments upon them.

The power of chloral to produce a kind of pains which are very likely to be called “neuralgic” by the sufferer, is an important fact to remember in reference to practical diagnosis.

Without wishing to make any sensational remarks (especially as the non medical public is apt to repeat such remarks with exaggerations), one cannot help seeing that the tendency to indulge in narcotism on slight pretexts is somewhat on the increase in society. Formerly, if Englishmen or Englishwomen had a certain amount of pain, or worry, or sleeplessness, they endured

<sup>1</sup> *Practitioner*, January 1873.

it, and hoped for better times—an opiate being considered a serious matter by most persons. At present the list of available soothers of pain and restlessness has become much larger; and from time to time fresh substances are discovered to possess the required power, and are at first stated to be also “quite harmless.” This was especially the case with chloral; but that drug has proved itself a wolf in sheep’s clothing with a vengeance. The public has not yet recovered from the belief in its harmlessness; and anyone who sees much of “nervous” practice must be aware that it is an exceedingly common thing, on being summoned to nervous cases of all kinds—slight or serious—to find that chloral has become a habit with the patients for some time past: in fact, it is taking the place of the frequent “whiffs” of chloroform which so many people used formerly to indulge in. This being the case, it may well be suspected, after the experience of the case which I have narrated, that a good many persons who complain of pains which they call “neuralgie” or “rheumatic,” really owe their sufferings wholly or in part to chloral; and this forms an additional reason for that rigid accuracy of diagnosis as to the true character of pain in every case, which I have repeatedly insisted upon. And as regards the chloral-mania, it is certainly quite as necessary to give a caution as it was with regard to the abuse of hypodermic morphia. I cannot help adverting here to another consequence of the injurious action of chloral, which I learned, only a few months since, in a fashion somewhat humiliating to myself. I was called, in consultation, to a patient who was suffering from symptoms of partial paraplegia, the origin of which it was difficult to make out. The patient was a lady of some 38 years of age, and had been passing through a great deal of agitation of mind. There was a suggestion that the paralysis was a so-called “reflex” effect of uterine mischief, but this theory broke down. The practitioner who had called me in suggested that the nightly dose (not large) of chloral, which the patient had been in the habit of taking for some time past, might be the cause; but I rejected this theory as altogether improbable, and, as there were pressing reasons, just then, for securing her some sleep, allowed the patient to continue the night-draught. The paralysis, which I hoped was a merely temporary functional affair, due to the nervous exhaustion which

she had endured, did not get any better from the remedies I recommended, and after a consultation or two another physician was called to see the case instead of me. He approved the chloral hypothesis, discontinued the night-draught, and had the satisfaction to observe the speedy disappearance of the paraplegia! I made some practical, though naturally somewhat rueful, reflections on this little incident.

It is to be hoped that before long some one will give us a complete account of the toxicological position of chloral; for there is certainly no drug introduced of late years concerning which more erroneous rubbish has been taught. It is exceedingly useful, beyond doubt; but it is far enough from being harmless, and it would be well for us to get a complete view of the extent of its powers of mischief.



## Reviews.

*A System of Midwifery, &c.* By WILLIAM LEISHMAN, M.D., &c.

*A Manual of Midwifery, &c.* By KARL SCHRÖDER, translated by C. H. CARTER, M.D.

### [SECOND NOTICE.]

THE theory which Schröder propounds respecting the mode of septic infection in puerperal fever is one which we sincerely hope may prove to be correct, since its practical bearings are obvious and most useful. According to him there is not that universal danger of infection, whenever the uterine fluids chance to be putrid after confinement, which most authors have assumed: on the contrary, the vessels of the uterus should universally be, and as a matter of fact they nearly always are, so occluded that there is no danger of their absorbing any of the poisonous material into the blood. It is only, says Schröder, when putridity exists within the uterus or vagina at or very soon after the moment of parturition that the danger of auto-infective absorption arises: such a case would naturally exist (1) where a dead child had been decomposing for some time *in utero*, after the rupture of the membranes and the consequent admission of air; (2) when long pressure had already induced gangrene of the soft parts before delivery; (3) in carcinoma of the cervix, when the new growth readily putrefies.

Upon the same principle, of course, septic infection from *without* will only be possible when the infection is brought (by the examining finger of the accoucheur, or in any other way) in contact with lacerated mucous membrane the vessels beneath which are still pervious.

Puerperal fever is thus without any specific qualities: it is a disease of the same general character as erysipelas, pyæmia, "ichorrhæmia," or "septicæmia;" and if there be anything special in its phenomena it is due altogether to the organ on which infection takes effect. Moreover, the time of possible infection

is on this view very limited, a fact which has very practical bearings, if fact it be; since it places in our hands the power very greatly to reduce the number of cases of auto-infection.

In the treatment of puerperal fever ought naturally to be included the prophylactic measures, because these are not merely of use in warding off the affection, but also in mitigating its severity when actually present. Schröder holds strongly the view that self-infection is far less common than infection from without: he deals first with the former. As a natural consequence of his belief that putrid matters of uterine or vaginal origin are only infective when they already exist at the time of labour or immediately afterwards, he makes the practical observation that we ought by all means to prevent this occurrence. For example, tedious labour is likely to be a cause of puerperal fever only when the bruising of soft parts has led to putridity before or immediately after delivery; *i.e.* before the uterine vessels have had time to become occluded. He therefore lays down the principle that we ought to shorten labour in every case where this threatens to take place. In all cases where there are putrid discharges before delivery, or where the performance of craniotomy or embryotomy has brought the parts of the foetal body in contact with the genital canal, we ought at once after delivery to disinfect the lacerated mucous membranes with dilute carbolie acid.

It is to us a matter of surprise that Schröder does not mention another source of auto-infection against which the very principles which he advocates ought to lead us specially to guard. It is at least well recognised by English accoucheurs that a very serious danger of infection arises from the too frequent negligence which fails to secure a complete and lasting contraction of the uterus after delivery. If it be indeed the case that (once the labour completed) there remains but a small period during which the uterine vessels are in such a state as to admit of infective absorption, then surely we ought to seek by every means to reduce the chances of such absorption during the hours immediately succeeding delivery. An all-important matter, therefore, is the sedulous attention of the practitioner to securing the immediate and firm contraction of the womb. We believe that a very large number of cases of infection, more especially those which have followed on protracted and exhausting labours, have been due to the fact that the uterus was permitted to remain in a flabby, semi-expanded condition, which must have left many vascular orifices patent to the poison. The inculcation of systematic kneading of, and continuous firm pressure by pad and bandage upon, the uterus, therefore forms an essential part of the directions for prophylaxis.

In regard to the external precautions destined to prevent

infection from without, there is nothing absolutely new in Schröder's directions, but they are noteworthy from their minuteness and conscientious care. The only debateable point is whether, even if he had followed them with mathematical accuracy, the practitioner would really (as Schröder says) "not be forced to give up his obstetric practice for a time, if puerperal fever happens to occur." Upon this point there are recorded facts which at first sight seem to make such a result hopeless, yet these may possibly be got over. For instance, there is an example recorded in Dr. Leishman's work in which two medical men who had had disastrous outbreaks of puerperal fever among their private patients, went away altogether for five weeks, during which the disease disappeared, but no sooner did they return than the infection broke out again. Here, however, it is abundantly possible that each of these gentlemen retained in wear some garment which became infected with septic matter, and was thus, in truth, as direct a carrier of poison as he had been before his supposed purification by abstinence from practice. And on the whole it seems to us most probable that, so far as the infective agency of the medical man is concerned, we may look forward to a time when the practice of thorough disinfection of person, clothes, and instruments will enable the practitioner to go on with his obstetric business quite safely even after the occurrence of puerperal fever in one of his patients. The only real safety seems to lie this way, and not in mere temporary abstinence from work. But assuredly, as regards *nurses*, there is (as Schröder remarks) no safety short of compelling them to abstain altogether from attendance on fresh labours, after attending on a case of puerperal fever; and their ignorance of the special sources of danger demands that they should be put under a quarantine of observation in order to secure the complete fulfilment of all proper precautions.

In the treatment which he proposes for puerperal fever when actually developed, Schröder is in some respects in advance of, in others (as it seems to us) considerably behind, the English point of progress. Thus he fully recognises and advocates the employment of the cold water treatment in cases with persistently high fever. But, on the other hand, he places an amount of reliance on purgative treatment which surprises us; and though we are bound to listen with deference to statements which are founded on large experience, it is impossible not to suppose that there is some error of observation. That this is the case is, we think, rendered all the more probable by the fact that Schröder believes in the possibility of material elimination of septic matter through the intestines; and all experience teaches that a physician once imbued with a seductive *à priori* notion like that of elimination, is from that moment in great danger of

observing in a biassed and consequently unreliable manner. However, it must be remembered that he cites, in favour of his opinion, the recent observations of Latour, Seyfert, and Breslau.

On the other hand, Schröder does not recommend venesection, even in severe peritonitis. But he speaks highly of leeches, and recommends mercury to partial salivation: a practice for which we cannot imagine the slightest justification. In England we have shaken ourselves free, and with most manifestly good results, of the baleful custom of mercurialising patients who are suffering from serous inflammations even of the common type; far more should we reject the idea of such treatment in inflammations of septic origin.

The work of Dr. Leishman is, in many respects, not only the best treatise on midwifery that we have seen, but one of the best treatises on any medical subject that has been published of late years. He says, very truly, in his preface, that a complete English work on the subject was really required, as no book existed that offered a complete account of modern knowledge on various topics immediately connected with parturition. He has brought several excellent qualities to his task. The style is easy and pleasant, the author's experience is obviously great, and there is an evident candour in his judgment of disputed questions. It is of course not in the province of this journal to discuss the physiological chapters of the work, but we cannot but remark, in passing, that these appear to us to be singularly well done, the subjects being presented in a plain and impressive manner, divested of much of the technicality which often makes them unattractive to students; at the same time a very thorough knowledge is both evidenced by the author and conveyed to the reader. The practical chapter on the management of labour strikes us as very good, while that on the mechanism of labour has the merit of giving a very clear explanation, not only of the difficulties of the subject, but of the course of inquiry by which true principles have gradually been acquired. We must confess to have been startled by the author's sanctioning such a practice as bleeding and tartar emetic for a rigid perineum, however; and it is a fact that throughout the book there is a tinge, however slight, of the ancient antiphlogistic views. In all that regards instrumental delivery, Dr. Leishman is full and clear, and we may take occasion here to say that the book is everywhere illustrated with drawings which admirably explain the author's meaning. As regards the question of the applicability of the forceps to cases in which the head is still very high up, Dr. Leishman takes a very different view to Schröder, and one which we believe to be far more practical and just. He does not in the least ignore the grave difficulties of the use of the



"long forceps" in the strict acceptation of the word—that is to say, in cases where the head will not enter the brim or descend beyond the upper part of the cavity—but he believes that the dangers of the proceeding have been greatly exaggerated, and ascribes a large proportion of the unfortunate results which have been obtained to the use of improper instruments. Among the chief faults which may make a long forceps unfit for its work, he reckons an over-slightness and weakness of make, and he emphatically endorses the observation of Barnes on the disastrous influence which a too weak instrument exercises upon the feelings and consequently upon the efforts of the operator.

In dealing with the subject of puerperal fever, Dr. Leishman displays very unequal merit in different parts. In all that concerns the pathology and causation of the disease he is clear and full, without being too elaborate; but there is a surprising amount of what we cannot but regard as feebleness of judgment in his manner of dealing with the therapeutics of the disease. We are prepared for this by the readiness with which he inclines to accept the doctrine of a "change of type" in puerperal fevers at various epochs—in the sense, we mean, of admitting a general "sthenic" character of these diseases at one time, and an asthenic at another. To our mind, and we suppose to the judgment of most physicians in the present day, the strong statements of such men (eminent though they were) as Gordon, Hey, and Armstrong, in favour of heroic treatment with the lancet, go very little way indeed toward showing that this treatment was really addressed to a disease of the same general character as that which we now recognise as puerperal fever, but distinguished from the latter by a great superiority in the strength of the constitution. Precisely the same arguments have been urged with regard to pneumonia and other acute diseases by the advocates of a "change of type" of disease, but the course of enlightened medical observation has substantially disposed of them; and no practical physician now seriously believes that it was either necessary or advantageous, forty or fifty years ago, to bleed for such maladies as a matter of course. Dr. Leishman does not, indeed, approve of indiscriminate blood-letting; still, it would seem from his arguments that among the cases (which are the majority) in which the more decisively septic character is not at first prominent, a large number would be most appropriately treated in the early stage by bold venesection. Here is the picture of the typical case for this treatment:—"A patient, of robust constitution, who has been exposed to contagion, complains, after a rigor, of acute hypogastric pain, which is accompanied by a rapid, incompressible pulse, throbbing temples, and a suffused countenance," *i.e.* one in whom the symptoms for the moment are those of peritonitis



or metro-peritonitis, with commencing general disturbance of the system, but without the symptoms of profound blood-poisoning. But while granting to the full that many cases between which and the true puerperal fever there is no ground for a substantial distinction, may be at the time, and may remain from first to last, for all practical purposes, mere local inflammations of the pelvic organs, we fail to see on what grounds a remedy like blood-letting can be recommended. One would say that in circumstances where the primary source of danger is the absorption of a poison, and the secondary chances of extension and increased gravity of the phenomena are dependent on such events as the occurrence of uterine phlebitis and the discharge of portions of dissolving and probably poisoned coagula into the general circulation, there is absolutely nothing to be hoped for in the way of limitation of the disease by abstraction of blood from the general venous system, while there are many possibilities of direct mischief from such a measure. If we are to look upon cases which at first exhibit little but local phenomena with some ordinary pyrexial disturbance as mere local affections, why are we recommended to deal with them on different principles from those which would guide us in the management of peritonitis from any form of internal traumatism independent of the puerperal state? And certainly it would never occur to ordinary physicians at the present day to bleed for peritonitis of the latter kind, while the majority would probably eschew even local abstraction of blood, and would rest their treatment entirely on the use of local sedatives (like ice), together with the free internal use of opium. On the other hand, when the septic character is declared, and more especially if the skin is very sallow or actually jaundiced, with sweating of a peculiar character which gives no relief to the high temperature, and the consequent oppression of the nervous centres, and if the respiration assumes that high pathognomonic rapidity which so especially belongs to pyæmic and septicæmic conditions, it would be in the highest degree dangerous, as Dr. Leishman himself remarks, to attempt any depletive measures whatever. [We observe that he has not a word to say in favour of attempts at elimination by the intestinal canal, which Schröder thinks so useful.] And our own belief is strong, that cases which stand at all near the border line where local mischief merges into general poisoning of the system are in nowise to be helped either by actual depletion of blood or by such use of purgatives as involves at once a considerable discharge of fluid from the bowels, and also (though this, unhappily, is usually forgotten) a determination of blood to the intestines. The removal of any fecal accumulations, by enemata, is a different and a very desirable proceeding. The thorough

employment of local disinfection is another obvious course, already sufficiently discussed. These things done, we may fairly believe that, until the advent of unmistakable symptoms of general septicæmia, a treatment directed solely towards securing physiological rest to the abdominal and pelvic organs offers all the chances of doing good that are really at our command. But if the septicæmic character of the affection becomes undoubted, we know no reason why the practitioner should for a moment hesitate to employ those remedies which are proved, both by physiological experiments and by the result of extensive clinical trial, to unite antiseptic qualities of the most undoubted type with a direct power over the migratory movements of corpuscles, and the multiplication of colourless blood-cells, which form at least the most important elements of the inflammatory process. The simultaneous employment of alcohol and quinine, the latter in no stinted quantities, appears to be as clearly indicated as possible; and we will add that if this treatment be only adopted in good time, and not as a miserable last resource, it will be found effective in no despicable proportion even of those cases which wear a very unfavourable aspect. It must be remembered, also, that modern experience tends increasingly to show that cases may be tided over into the chronic stage, and that with proper nutriment and hygiene, and our greatly improved means of evacuating purulent effusions from joints and from serous cavities, the expectation is not unreasonable that at no very distant date we shall be in a position to save the majority of such cases. In fact, to sum up, there are no other than local reasons for thinking of puerperal fever as different from any other septicæmic infection, and beyond local precautions there is no rational treatment for the disease except the use of remedies which can act as direct antiseptics and antiphlogistics in the manner of alcohol, quinine, and the other substances which recent researches have shown to be allied to these. It is worthy of remark, in this connection, that camphor, which was the basis of the treatment successfully employed by Copeland, has been recently proved to belong to this group of remedies.

We have pleasure, in conclusion, in recommending the work of Dr. Leishman as a treatise of the highest worth, and one which will be found of constant practical service to all medical men who are engaged in midwifery. It is no small merit of the book that, besides being clearly and elegantly printed and neatly got up, it hits with accuracy the mean between bulkiness and slinness, no less in outward form than in internal execution.

*Ueber das Amylnitrit und seine therapeutische Anwendung.*  
Inaugural Dissertation. Von ROBERT PICK. Bonn, 1874.

THIS treatise is a worthy successor of the many important monographs on the action of particular remedies which have issued, during the last few years, from the therapeutic laboratory at Bonn. It commences with a discussion of the chemical characters and relations of amyl nitrite, which contains some interesting points, but which need not detain us. The second or "clinical" chapter is exceedingly interesting; it is a careful collection of all the most interesting English as well as German observations on the therapeutic employment of amyl nitrite; and to ourselves it gave much new information. We are not yet converted to the belief that amyl will do all that many persons, including Dr. Pick, seems to expect of it; but after reading the evidence which he has collected, we shall feel it our duty to give a more serious trial to it, in epilepsy, for example, and in tetanus, if we get the opportunity of doing so. Upon one point as to which Dr. Pick has gathered evidence we are able to speak with a good deal of experience, and have come to conclusions which are probably of value; we refer to the treatment of migraine by amyl. Several cases are given by the author in which this malady was greatly palliated, and a few in which it appeared to be cured. Now, migraine is a disorder that is constantly coming under our notice, and the question of the probable utility of amyl was of course very early suggested to us by the well-known experience of Du Bois Reymond. Of course it is only in examples of the disease which resemble Du Bois'—viz., those with pale face and evidently contracted blood-vessels—that amyl would be thought of. In such cases, however, it did seem very reasonable to try amyl, and accordingly we long since began to experiment with it, and were the more prepossessed in its favour because we had obtained a remarkable success with it in angina pectoris. But even in the "spastic" cases of migraine we have not obtained successes that are of much value. At first it seemed otherwise; in one or two patients the production of the well-known physiological effects of amyl was followed by such an instantaneous relief to the pain as was remarkable, and one such patient got a complete immunity from the attacks, after one or two inhalations, which lasted nearly a month, during which time, according to her regular custom, there should have been at least two bouts of migraine. Of course, if one thinks of it carefully, such a success was far *too great*; it was highly improbable that the direct influence of a dose or two of a volatile drug like amyl could produce such a change. Subsequent experience has quite confirmed this idea, and indeed the failure of amyl in migraine has

been one reason which has much contributed to our present decided opinion that vaso-motor changes in migraine are quite secondary, and not primary or causative. No doubt a sudden change in the circulation like that induced by chloral, where there has been vascular spasm, must produce a great effect, not merely physical but mental; and the influence of *surprise* upon pains of a neuralgic character is proverbially known. But such effects are for the most part transitory, and if the process be frequently repeated it loses its force. And accordingly we have found that persons who at first seemed to derive much benefit from amyl very soon ceased to have any confidence in it as a remedy for migraine.

A malady in which, next to angina pectoris, amyl nitrite appears to have the most decidedly beneficial influence is spasmodic asthma; another illustration of the close pathological connection which exists between the two diseases. There is much evidence on this subject, both from Dr. Pick and from others whom he quotes; and it certainly does appear that henceforth a large number of our asthmatics may be far more speedily relieved in their terrible paroxysms than by any previously known drug. It has yet to be proved, however, that this influence can be continued during a series of years as we know it can be<sup>1</sup> in angina pectoris.

In regard to the employment of amyl for tetanus, there is the same doubt in our minds that hangs over its use in epilepsy. The very striking case related by Dr. Pick himself gives an illustration of what we mean: the first series of inhalations produced a marvellous effect both in relaxing the spasms and in relieving the general state, and yet, as it were without rhyme or reason, all the bad symptoms suddenly returned in their worst form. Still, it is undoubtedly possible that were the remedy applied with diligence at a very early stage (Dr. Pick's patient had been at first treated with curara), the effect might have been so great that the danger would have been tided over; and unquestionably, amyl nitrite remains one of our most promising remedies in a disease where our hopes can only be very moderate at best.

The original experiments on animals and on man by Pick are very interesting; they lead him to announce the following conclusions:—(1) Amyl nitrite produces a general torpidity of the whole muscular system, but it especially affects the organic muscular fibres. (2) The latter effect is especially to be recognised in the muscular fibres in the blood-vessels. A few drops of the amyl suffice to produce a rapid and inevitable dilatation of the vessels, more particularly of the upper parts

<sup>1</sup> See Dr. Madden's history (two years), for example, in *Practitioner*, vol. ix. p. 331.



of the body, with a simultaneous lowering of arterial pressure and hurrying of the heart's action. (3) This influence probably depends on a direct action upon the unstriated muscular fibres in the vessels. (4) In consequence of this action of amyl nitrite, it becomes a valuable remedy at once for all those diseases which depend on a spasmodic or excessive contraction of the vessels; and also, perhaps, which are produced by spasm of other muscular tissues, whether smooth or striped; especially also for many cases of hemicrania, angina pectoris, epilepsy, eclampsia, asthma, trismus and tetanus, and the like, in which it is, however, to be remarked that in many cases amyl only acts as a palliative. Perhaps the numerous convulsive conditions of childhood offer a rich field for the operation of amyl nitrite; but so far as I know there are as yet no observations on the subject. It is interesting to see that as regards the main subject of the direct action of amyl upon the muscular fibres of the blood-vessels, without the intervention of the nervous system, Pick confirms the admirable researches of our own Brunton: he makes some interesting observations upon protozoa, which consist of a mere contractile non-nervous substance, and gets decided results in prevention of the contractile movements. Among many other interesting matters in Pick's research is the demonstration that (notwithstanding all that has been said to the contrary) the retinal vessels remain absolutely without share in the process of dilatation which is so evident in those of the face.

The author concludes his pamphlet with some practical remarks on the best method of administering amyl, which will doubtless be of interest for our readers.

"The best mode of use is inhalation. A few drops are poured on a towel, or, as others recommend, on blotting paper or wool, and placed over the nose and mouth. There are other modes of application besides inhalation: use by the stomach, and by subcutaneous injection; but the latter would seem from some observations to be less effective than inhalation, which is therefore to be preferred. Moreover, in this way we avoid upsetting the digestion. If given by the stomach, 2 to 5 drops must be administered on sugar. In inhalation we commence with 1 or 2 drops, and gradually increase to 5, 10, or 15 drops for a dose. The latter quantity is necessary after prolonged use, as in all probability amyl, like the narcotics, very easily sets up a habit of tolerance. The necessity, in epilepsy, *e.g.*, of having the remedy close at hand, yet without allowing a great quantity to be thrown upon the respiratory organs, gives to the following procedure, employed in the garrison hospital here (Bonn) by permission of Dr. Strassburg, a practical recommendation:—

"Dry charpie is placed in a glass, and the proper dose is



dropped upon it; the vessel is then closed by a stopper made absolutely air-tight by means of paraffin. The patient, when he feels any warnings of an attack, can rapidly open the glass and inhale the measured quantity without danger. It is of course to be understood that in that way of using it the remedy must be often renewed, owing to its great volatility and liability to decomposition."

This treatise of Dr. Pick's is both interesting and valuable, and above all it is an indication of the superior manner in which medical education is carried out in the German Universities. We hope that the time may soon come when in our own medical schools it will be the rule, instead of the highly rare exception, that advanced pupils will before "finishing their education" have executed some piece of original inquiry under their teacher's eyes. It must surely be of incalculable value to an observer that in his first steps in the path of original research he shall not be left to stumble about without guidance or help.

## Clinic of the Month.

**The Detection and Removal of Vesical Calculi.**—Mr. W. D. Napier, in a paper read before the Medical Society of London, suggests a new form of sound for the purpose of detecting the presence of a calculus with more certainty than by the means at present in use. He calls it the "Calculus Detector," and describes it as being precisely similar in form to the ordinary sound. It is composed of steel up to the commencement of the curve, where it is reduced in diameter, and a coating of pure lead is cast round the smaller part, of sufficient thickness to render the surface perfectly smooth and even along its whole length. The leaden extremity is then polished as highly as possible with a leather, and rendered keenly sensitive to the slightest contact with any rough or hard substance, of which it would be now found to bear visible trace. With regard to the "Calculus Extractor," he is inclined to confine its use to the removal of small stones and fragments of stone. He describes it as an instrument intended to withdraw from the bladder either a stone intact or crushed into fragments, so enveloped in a delicate elastic wrapper—for it literally folds itself round the substance in question—that injury to the passage is simply impossible. It consists of a soft india-rubber tube, terminating at one extremity in a funnel-shaped orifice, greatly resembling the ordinary convolvulus flower, and it is capable of containing a calculus or portion of a calculus of considerable dimensions. When it is inserted into the bladder, its form and position render it the receptacle of such foreign substances as would, from the position of the patient and the flow of urine, determine towards the natural outlet. For a long time Mr. Napier was perplexed as to the mode in which the india-rubber could best be introduced into the bladder. His first idea was to insert the convolvulus-shaped cup folded in the form of a bud in twisted æstivation, and retained in its position by some adhesive substance that would permit it to expand after its introduction. But there were several objections to this, and at last it occurred to him that, inserted in a fine silver canula, it would be utterly

at the direction of the manipulator; but a new difficulty arose as to the mode in which a canula with open mouth could be conducted into the bladder through the urethra. This, however, was overcome by making an artificial point with cocoa butter, which, whilst it is a hard dense substance, melts with great facility at  $96^{\circ}$  Fahr. There was the additional advantage that it serves as a lubricator. (*British Medical Journal*, Dec. 20, 1873.)

**Dr. M'Kendrick on Ozone.**—Dr. M'Kendrick read a paper giving an account of an experimental investigation made by Dr. James Dewar and himself on the physiological action of ozone, at the Royal Society of Edinburgh, at the first ordinary meeting of the present session, held on the 1st instant. Dr. M'Kendrick described, in the first place, the method pursued by Mr. Dewar and himself in producing the ozone. He then detailed numerous experiments made on frogs, mice, birds, rabbits, and on Dr. Dewar and himself, with the view of determining the action of ozone on the living animal, and the action it exerted on the living tissues of the body. The general facts observed were—(1) The inhalation of an atmosphere highly charged with ozone diminished the number of respirations per minute; (2) the pulsations of the heart were reduced in strength, and this organ was found beating feebly after the death of the animal, which showed that life was not destroyed by direct action on the heart; (3) the blood was always found in a venous condition in all parts of the body, both in those cases of death in an atmosphere of ozonised air and of ozonised oxygen; (4) ozone exercised a destructive action on the living animal tissues if brought into immediate contact with them, but it did not affect them so readily if they were covered by a layer of fluid; (5) ozone acted as an irritant to the mucous membrane of the nostrils and air-passages, as all observers had previously remarked. These effects the authors attributed partly to the fact that the density of ozone was slightly more than that of the carbonic acid of the blood, so that the diffusion of the gases in the air-cells of the lungs was retarded, and there was a consequent accumulation of carbonic acid in the blood. At the same time ozone had also a subtle effect on the blood which further experiments might discover. The authors exhibited the apparatus employed in the experiments, and showed how ozone could be readily produced. The apparatus consisted of arrangements by which a diffuse discharge of electricity from a powerful induction-coil could be sent through a stream of air or of oxygen passing through a glass tube, so that during the action of the coil ozone issued freely from the end of the tube. (*Medical Times and Gazette*, Dec. 20, 1873.)

**Removal of Growths, etc., by Elastic Ligature.**—Sir Henry Thompson describes the case of a woman whose right breast he has recently removed by means of an india-rubber ligature, and who has made an excellent recovery. The progress of the case was unsatisfactory at first, owing to two circumstances. The first was an attack of erysipelas, prolonged rather than severe, which appeared two or three days after the application of the ligature, and which affected the right arm, shoulder, and most of the back; but there was no reason to suppose that there is any special liability to erysipelas from the peculiar nature of the operation. The second unfavourable circumstance was, that being Sir Henry's first case the elastic thread was not tied tightly enough. At the end of eight or ten days the ligature was evidently not embracing the still undivided portion so firmly as it ought to have done. He therefore made it tighter, which was very easily done by pulling out the loop from the wound as far as possible without much hurting the patient, and tying round it a fine ligature so as to diminish perhaps by one half the encircling noose. The mass was larger than was anticipated, but it was entirely removed, and the wound rapidly healed. In this case india-rubber tubing was used, but Sir Henry thinks that it was not sufficiently strong, and has had some solid cord made which is much more powerful, and which he believes will divide the tissues in at least one-half the time. It can be obtained from Messrs. Weiss and Son. Sir Henry thinks it is better, as Professor Dittel now does, to apply the ligature to one half the breast at a time—the lower side—and not to use the second for the upper half until the first has separated. The principal objection to the plan is the smell which necessarily arises from the sphacelated portion. This in the case reported was much controlled by constant irrigation with carbolic acid and water, from a bottle placed above the level of the patient's head. (*Lancet*, Jan. 3.)

**Treatment of Suppurating Glands.**—Professor Crocq, of the University of Brussels, in a recent clinical lecture (reported in the *Presse Médicale Belge*, November 23), described the treatment which he has been in the habit of pursuing during the last ten years in suppurating glandular abscesses. Situated as these usually are in the neck, unsightly scars are often produced, even when they are treated as recommended by Velpeau—by a narrow puncture executed as soon as fluctuation is apparent, and repeated if necessary. Even this leaves a scar, which indeed may become very conspicuous when, as often happens, the little aperture ulcerates and become fistulous. Professor Crocq avoids these inconveniences in practising the puncture by means of a grooved exploratory needle or a fine

exploratory trocar. Pressure is then made over the swelling so as to expel as much matter as possible. If the abscess is extensive, or the fluid does not issue freely, he makes two, three, four, or five punctures at different points, repeating them daily or every other day, either at the same or at other points. Sometimes it is sufficient to press the tumour somewhat firmly in order to secure the flow of the pus from one of the punctures already made. After some days the matter becomes less abundant and more serous, taking on a reddish tinge, which is a sign of an approaching cure. Sometimes there is now observed a more or less considerable soft and doughy swelling which if punctured only yields a little blood and serosity. This swelling soon disappears, and the punctures, which at first remained as little reddish points, soon become pale, so that after the cure of the abscess no traces of them remain. This procedure is employed for acute abscesses as soon as fluctuation has become evident, and in chronic cases before thinning and detachment of the skin has occurred. But even when this condition exists it is not a contra-indication, care being taken, however, not to puncture the most thinned portions of the skin, in fear of subsequent ulceration. After the punctures these parts fall in and retract, a depression as from loss of substance resulting; but in general this usually disappears with the lapse of time. Even if it did not do so, its aspect would be far less repulsive than that of a cicatrix due either to a natural or artificial solution of continuity. Professor Crocq has never found ulceration succeeding these punctures. He points out that his plan is different from those of Mr. Lawson Tait and Dr. Lorentzer, which combine the employment of aspiration with minute puncturing; and he prefers his own as more simple and more efficacious.

He has also employed the same procedure in opening suppurating bubo, avoiding thus the disagreeable scars that are usually left. Applied in good time, when the collection has well formed, and before the skin has become thinned or detached, it always succeeds, except in the case of "chancreous bubo," when the presence of a virulent pus necessarily leads to ulceration. In the great majority of cases the bubo is a simple inflammatory tumour.

Any subcutaneous abscess which is not of very large extent may be treated in the same manner. Thus M. Crocq has frequently met with great success in treating abscess consequent on erysipelas of the face, and especially in the abscesses consecutive to variola, of which he met with a great number of cases during the epidemic of 1865.

Several cases are given in illustration. (*Medical Times and Gazette*, Dec. 1873.)



**Ascites from Obstruction in the Portal Vein.**—Sir W. Jenner gives the details of the case of a man whose abdomen gave way at the umbilicus with a loud report, in a clinical lecture delivered at University College Hospital. In this case the gas was in the peritoneal cavity, and he had previously been tapped; the wound did not heal, and when the gas rapidly accumulated the weak spot ruptured. The real causes of the man's troubles were an impediment to the flow of blood through the portal vein, and an impediment to the escape of bile from the hepatic duct. Sir William describes in succession the mechanical consequences of congestion of the portal system, both when gradual and when sudden, and then gives the history of the patient, who had been in India and indulged from time to time in drinking bouts. He had been tapped, had had hæmatemesis, and suffered from perforation of the bowel, which led to peritonitis. (*Lancet*, Jan. 3, 1874.)

## Extracts from British and Foreign Journals.

**Treatment of Chronic Coryza.**—Dr. Diruf, sen., of Kissingen, observes that not only is chronic catarrh very troublesome to the patient, but that, owing to its accompaniments and sequelæ, it is often dangerous. It is not unfrequently associated with an eczematous lupus-like cutaneous affection of the nose, polypous growths and ulceration of the mucous membrane, with necrosis of the nasal cartilages and bones, with extension of the inflammatory processes to the accessory cavities of the nose, to the pharynx, and Eustachian tube. Usually the coryza chronica ulcerosa is accompanied by the disagreeable odour of decaying bone ("*putrescent*"). Amongst the more important sequelæ of the affection are, that in consequence of contraction and occlusion of the nasal canals the secretions are retained in the frontal sinuses and in the antrum, in which suppuration may take place; the abscess in the former cavities may even burst internally through the bones into the cavity of the skull. In regard to the treatment that should be adopted, it should be in the first place generally anti-scorfulous; but the local treatment is much more important. Dr. Diruf strongly recommends the employment of Prof. Weber's nasal douche, which consists of an india-rubber tube about a yard and a half long, having a perforated nut at one end which can be inserted into the nostril, and through which a current of fluid can be driven whilst the patient inclines his head a little forwards. The fluid then passes, not through the choana into the pharynx, but, after thoroughly washing all the surfaces of one nostril, through the other nostril, which remains open, and from which masses of inspissated mucus are often expelled. Dr. Diruf has made the nasal nut conical in form, which permits it to fit the nostril more accurately. The inclination of the patient's head should not be too great, as the fluid may then be driven into the frontal sinuses, which causes severe pain and frontal headache lasting for several hours. Instead of pure water, weak solutions of various salts may be employed, and Dr. Diruf recommends about a  $1\frac{3}{4}$  per cent. solution of the salts of the Kissingen Bath. The

pain over the glabella, often experienced in coryza chronica, disappears after the use of the nasal douche. (*Der Praktische Arzt*, No. 10, 1873.)

**Epidemic Influenza.**—Dr. Gerard G. Tyrell, of Sacramento, gives an account of an epidemic of influenza that has affected the district of Sacramento during the last year. The first patients complained simply of a cold in the head, frontal headache, defluxion from the eyes and nose, pains in the limbs, and sometimes cough. This, with a feeling of lassitude and a desire to abstain from bodily and mental exertion, characterised the invasion of the disease. In these cases herpes labialis was a constant symptom. A very short time afterwards patients presented more serious symptoms; they were seized with an initial chill of shorter or longer duration, which was followed by fever, which generally manifested itself within forty-eight hours of the initial chill; early and sudden prostration of strength was almost invariable; as evening approached, flashes of heat or continued fever succeeded the rigor, with headache accompaniment, suffusion of eyes, smarting of eyelids, with a feeling as if "the nose was stuffed," dryness of mouth and throat, with occasional cough, intolerable pains in the back and limbs, great thirst, sometimes vomiting, and always lassitude. Within about twenty-four hours the feeling of chilliness ceased, and a continued fever of mild type prevailed. Tongue white and creamy, pulse rarely over 100, and the temperature from 99° to 102° F. Cough, if not an initial symptom, now presented itself, hard, dry, and paroxysmal, with a feeling of rawness beneath the sternum and along the trachea. With this was an invariable complaint of sore throat, which presented a dusky redness of the fauces. The bowels were constipated. The urine was scanty, dark red, and acid. Towards morning the skin would become moist, and often perspire freely, but without relief to the muscular pains. During the forenoon the patient always felt better: the pulse would perhaps drop to 80, the temperature to 99°, the headache abate but not entirely cease; the cough would still continue hard, dry, and paroxysmal. As evening again approached, the headache, fever, pains, and thirst would return, sleep be disturbed, and the sweating be renewed. Generally upon the fifth or sixth day all the symptoms would be found to have abated except the cough, which would be accompanied by the expectoration of thick viscid mucus. On the eighth day convalescence would be established. In another type of the disease all the symptoms would be aggravated on the fifth or sixth day. After a very restless night with wild delirium, the morning would find the pulse up to 110 or 115, the temperature 102° to 104° F., the tongue dry and parched, the

cough aggravated, and the breathing painful; auscultation would reveal pleuritic frottement. Perhaps the next day under treatment the pain would have abated, and bronchial râles be developed over the chest. Still oftener, within twenty-four hours consolidation of lung would have occurred, with high temperature, frequent pulse, and great prostration. These latter cases proved very fatal. The epidemic seldom attacked old men. Diarrhœa was not noticed as a common complication. The treatment that seemed to be the most successful in Dr. Tyrrell's hands in abridging the duration of the muscular and rachitic pains, in allaying fever and promoting expectoration, was a mixture of nitrate of potash, tinct. aconite, spt. of ether, syrup, and liq. ammon. acetatis; followed, when the initiatory symptoms had subsided, by muriate of ammonia, quinine, and compound elixir of cinchona combined. When pleurisy supervened, he found nothing to allay the pain so speedily as a well-made and large-sized blister. In bronchitis the muriate of ammonia in fifteen to twenty grain doses did efficient service, both by promoting expectoration and thinning the viscid sputa. In the pneumonia complicating this disease, quinine in full doses was found to be the most reliable agent of any used in promoting resolution, and lowering and giving tone to the pulse. Stimulants were found to be also good allies in battling with asthenia, and were generally prescribed either in the volatile form, as carb. ammonia or chloric ether, or in a more persistent shape, as whisky, brandy, egg-nog, &c. In the few cases in which diarrhœa supervened, tannin, either combined with bismuth or added to the quinine mixture, generally fulfilled the indications desired. Opium was seldom needed, and refrained from as much as possible, as it dried up the secretions and produced headache. Hydrate of chloral was often used, without any apparent benefit. In those cases in which sweating was excessive, Dr. Tyrrell found the old-fashioned remedy of sage tea a most trustworthy agent, whilst the mineral acids and oxide of zinc met with most signal failure. Where headache was a prominent and persistent feature, fifteen or twenty grain doses of the bromide of ammonium or potassium usually gave speedy relief; it also seemed to possess some influence in mitigating the troublesome coryza, and in some cases appeared to arrest this symptom; it also allayed the paroxysmal character of the cough when continued to be given for some time. Where pneumonia or severe bronchitis was present, the jacket poultice was universally employed, sometimes the plain poultice, but often moistened with turpentine or tinct. of opium. (*Pacific Med. Journal*, No. 5, 1873.)

**Treatment of Caseous Epididymitis and Tubercle of the Testis.**—For many years past it has been customary to



apply the term tubercle of the testis to a disease which, though less intimately connected with pulmonary phthisis than tubercular inflammation of the serous membranes, is not the less to be regarded as closely associated with it. Mangin, who has recently written an essay upon this subject, proposes to apply the term caseous epididymitis instead of tubercle of the testis. Is this, says M. Malherbe in a review of M. Mangin's work, an appropriate term and one that it is advisable to adopt? The term caseous inflammation, so far as it is applied to the testis, would only be of value in two points of view; first, and chiefly, supposing it to be true, were it to establish a distinction of the highest importance clinically between a disease serious enough, it is true, but curable, and the tuberculisation which is almost a condemnation to death; and secondly, were it a mere question of scientific dilettanteism, the new term designating precisely and exactly a process of pathological anatomy. In either case it might be worth accepting that suppuration of the epididymis, in consequence of its caseification, can lead to the production of pulmonary phthisis. Let us see whether caseous epididymitis fulfils either of these conditions. Is it true that caseous inflammations differ from tuberculosis in their nature, and can they merely predispose to it as Niemeyer paradoxically maintains? The Germans say yes, the French say no. Clinical experience appears to be with the French. Looking at it from the latter point of view, notwithstanding that statistics have been obtained showing that in fifty cases of tuberculous testis there was only one of pulmonary tubercle, it may be doubted whether such a ratio is of ordinary occurrence; and M. Malherbe thinks that he has seen many cases where the two diseases have been concomitant, and that those who are of a different opinion may have seen cases at a time when tuberculous disease, or at least when the symptoms of tuberculous disease of the lungs, had not yet made their appearance. In regard to the consecutive development of pulmonary phthisis, M. Mangin and Prof. Richet are in accord; for the latter remarks, "If the glands of the neck in young persons be allowed to suppurate for an indefinite period of time, a general tuberculisation may be induced:" whilst the former believes that epididymitic suppuration consequent on its caseification is capable of causing pulmonary phthisis. Is not this pushing the idea of *post hoc ergo propter hoc* too far, and would it not be more conformable to sound views of general pathology to regard caseous degeneration either of the epididymis or of the ganglia of the neck as lesions due to tuberculosis developing itself by producing changes in various organs before attacking the lung? Let us now see whether pathological anatomy does not demonstrate that the affection of the testis we are now considering is



really of a tuberculous nature. To guide us in this inquiry we cannot do better than refer to the works which have recently issued from the histological school of the College of France, that is to say, to the researches of M. Thaon and M. Grancher. Both of these authors maintain the unity of pulmonary phthisis. Caseous inflammation, says M. Thaon, "is a manifestation of tuberculosis of the same value as granulation. True caseous pneumonia does not exist." In the absence of any histological investigations on the stages of caseification of the testis, it is reasonable to regard it as being, like caseous pneumonia, dependent on tuberculosis, and to hold it to be a tuberculous affection of the testis. One of the arguments against this view—to the effect, namely, that caseous inflammation developing itself at the expense of the epithelium of the spermatic ducts cannot be true tubercle, since tubercle always develops in connective tissue—falls to the ground before recent histological research. For M. Thaon has demonstrated that tubercular granulation may arise in the alveoli of the lungs at the expense of the epithelium. It would therefore appear to be premature to change the name of tubercular disease of the testis to caseous degeneration of this organ.

And now in regard to the treatment. The theses of MM. Auboin and Bouclage, which have just been published, discuss the value of the treatment by the actual cautery proposed by M. Verneuil, by which the operation of castration is avoided, but many facts must be accumulated before any definite conclusions can be drawn. It is important in all instances when the diagnosis is not absolutely certain to try the effects of iodide of potassium. Experience has shown further the advisability of removing the patient from all crowded rooms, which seem to exert so powerful an influence on the development of tubercle. Hence soldiers should be sent home, and the inhabitants of towns should be sent into the country. In brief, all the hygienic means employed for the cure of scrofulous diseases should be employed. What now are the surgical measures which should be used when the tuberculisation has invaded a large part of the epididymis, hollowed it out into caverns, and given birth to abscesses? Strumous abscesses of the testis have been opened either by a cutting instrument or by caustics, and when the pus is evacuated efforts are made to cleanse the interior of the cavities and promote cicatrisation by the use of astringent or alcoholic injections (Bermond of Montpellier); tincture of iodine (Bouisson), &c. More energetic means, as caustics, have also been employed to destroy the affected parts completely, as the paste of Canquoin (Bonnet; Maisonneuve), caustic potash (Thierry). The latter recommends the caustic potash to be introduced into the centre of the abscess in order that the adjoining parts may be destroyed from within outwards. These

methods of treatment are, however, either incomplete, or long and imperfect. But when it has been determined to treat the disease energetically, it is important that it should not be protracted, since confinement in bed is of little use to a patient suffering from tubercular testis; they require activity and open air. M. Chassaignac, the inventor of the drainage method, has employed that method in the treatment of tuberculous abscess of the epididymis, but this also requires much time. Malgaigne, wishing to do something more than merely make an incision, and yet to avoid castration, proposed in 1851 resection of the affected parts, but few surgeons have adopted it. It may, however, be tried if there be only a fistulous track that is capable of removal without making a large wound and exposing the patient to serious traumatic accidents. The method of cure suggested by M. Verneuil, by the actual cautery, may be adopted in cases of moderate severity, those that are very severe being reserved for castration. The actual cautery preserves a part of the testis, sufficient perhaps to satisfy the mind of the patient, whilst at the same time it avoids the dangers of castration, which, according to M. Perrin, carries off about one-fourth of all the patients. The principal objection raised by those who adopt castration is that by removing the testis the progress of the disease is radically cured. But M. Verneuil replies, very justly, that contrary to appearances, castration often proves only a palliative remedy, since tuberculation of the internal generative organs, vesiculæ seminales, prostate, &c., is often contemporaneous and even anterior to the tuberculation of the testis. *En résumé.* In a large number of cases the actual cautery will render valuable services in the treatment of tubercle of the testis, but it obviously cannot replace castration, which must always be adopted in severe cases. No absolute rule, however, can be laid down; surgical tact must decide which plan must be tried in any given case. (*Le Progrès Médical*, No. 26, 1873.)

**Hydrate of Chloral in Puerperal Eclampsia.**—Dr. Bourdon, having a case of eclampsia in a woman aged 21 years, determined to try the effects of hydrate of chloral. The patient had arrived at the termination of her first pregnancy, and for the last fifteen days had suffered from cedema of her lower limbs and eyelids, cephalgia, somnolence, and a large quantity of albumen in the urine, when she suddenly fell into a violent convulsion, which lasted for ten minutes. As soon as the attack passed off, M. Bourdon administered an injection per anum of 60 grains of hydrate of chloral; this induced immediate sleep. On the following morning, to prevent any recurrence of the attack, two more injections were prepared with 60 grains of chloral in each. The first was administered at 10 A.M., notwithstanding that deli-

very had commenced. Two hours after a second was given, and at 3 P.M. delivery was completed without consciousness on the part of the patient. In the evening, about 8 P.M., a fresh convulsion occurred, for which a draught of 60 grains of chloral was again given. She fell into a sound sleep, and the subsequent recovery took place without anything remarkable. (*Lo Sperimentale. Fascicolo nono, 1873.*)

**Hooping-cough as a Cause of Spinal Caries.**—In a paper on this point by Dr. Lee, of Philadelphia, he remarks that the fact of hooping-cough proving occasionally a direct cause of inflammatory and subsequently of ulcerative action in the spinal column, appears to be entirely overlooked by systematic writers on surgery; and this he attributes, on the one hand to the idea being generally entertained that Pott's disease is of strumous origin, consisting essentially in a deposit of tubercle in the body of one or more vertebrae, and on the other to the relation of hooping-cough to the disease not being easily recognisable by the professed orthopedist. Dr. Lee gives several cases, of which the following is one:—

“*Case I.*—E. P., native of Pittsburg, Pa., parents both living and in good health; aged ten years; is very fair, has flaxen hair and blue eyes. Has always been rather a delicate child, but has had no serious illness except diphtheria at the age of three years, from which she recovered perfectly. About a year ago, however, she was attacked with hooping-cough, from which she suffered very severely. On recovery from this attack, she was observed to stoop much more than had been her habit, and soon afterwards to lean somewhat towards the left in standing and walking. She was brought to me Jan. 29, 1867. Her condition at that time was as follows:—The spinal column deflected suddenly to the left from the sixth dorsal vertebra upwards; the deviation being angular in its character, but without posterior projection. There was no secondary or compensating curve above or below. The spinous process of the first dorsal vertebra was displaced to the right about half an inch. The suprascapular muscles on the right side were much relaxed, allowing the scapula to slide down the back at least an inch lower than that of the opposite side, and giving an appearance of greatly increased deformity—causing an apparent projection of that shoulder, which rendered the similarity to a lateral curvature very striking. The right leg was a quarter of an inch shorter than the left, and the toes of the foot were directed inwards. She was very easily fatigued. Her sleep was disturbed, her appetite variable, and her temper irritable. Having had occasion, a short time before, to witness an excessive aggravation of a case of already existing spinal caries, in consequence of the

intercurrence of hooping-cough, and being able to trace the present incipient deformity to no antecedent injury, I felt that I was justified, in view of the sequence of the diseased conditions and symptoms, in concluding that the one had been actually developed by the other, and, but for it, might never have occurred. I applied a spinal splint, so adjusted as to give support at the point of angular deviation, and prescribed a course of 'movements' or exercises especially designed to develop the dorsal muscles of the right side, more particularly the levator anguli scapulae and the rhomboidei. Under this mode of treatment I was gratified to see a steady and progressive, though not rapid improvement, which terminated in complete recovery, and in so doing convinced me of the accuracy of my diagnosis."

After detailing four other cases he continues:—

"It cannot escape notice that there are certain strongly-marked features common to all the cases: all present the history of a previously healthy child, or one at least presenting no indications of strumous diathesis, and whose parents were in good health, being attacked with symptoms of spinal inflammation shortly after an attack (unusually severe) of hooping-cough. No other cause could be assigned for the development of the spinal affection, in the way of external injury. The progress of the case was steadily towards deformity, and that deformity in every case, while evidently dependent upon inflammatory ulceration and loss of substance, had a strong tendency to assume a lateral direction. Although strictly angular in its character, it did not present any very considerable posterior projection, some influence evidently being at work to draw the spine toward one side. In two of the instances this lateral deviation was so abrupt as almost to appear like a dislocation. The inference is to my mind irresistible that there is in these cases more than a coincidence; that we have here a peculiar type of spinal deformity resulting from inflammation produced by the violent succussion of the intervertebral discs from muscular contraction in the terrible spasmodic cough of pertussis, aided, it may well be, by a dyscrasia resulting from the combined action of the poison of the disease upon the blood and the deterioration of that fluid in consequence of the impairment of nutrition due to the constant rejection of food which is so constant a feature of the disease.

"Two practical lessons attach themselves to this view. The first, the importance of carefully watching the convalescence of every case of hooping-cough, and taking warning on the very first appearance of any symptom indicating an implication of the spine. The second rather, perhaps, a suggestion: whether something might not be done, during the progress of a severe case of this disease, to protect the spine from shock. We all



know how instinctively and anxiously the little sufferer seeks anterior support the moment it feels a paroxysm of cough coming on. May we not take a hint from this natural desire, and by applying some simple form of support to the spine, at once give relief to present distress, and ward off impending danger?

"NOTE.—The tendency to lateral complication of the angular deformity above referred to is not easy of explanation. Two solutions suggest themselves: first, that one lung has become partially solidified by intercurrent pneumonia, thus interfering with the play of the intercostals, and inducing rigidity and contraction of the muscles overlying them; and, secondly, that, for some reason not understood, the expulsive action of the expiratory muscles of one side has been more powerful than of those of the opposite side, thus producing greater injury to the articulating surfaces of the vertebrae upon that side." (*Cincinnati Medical News*, No. 11, 1873.)

**Therapeutic Value of the Eucalyptus Globulus.**—Dr. Burdel, of Vierzon, gives an account of a second series of researches he has undertaken on the action of the eucalyptus as a febrifuge. The cases were thirty-three in number, eleven of whom suffered from quotidian fever, twelve from tertian, and ten from quartan. Of the eleven who suffered from quotidian fever, four were cured and seven were not cured; of the twelve cases of tertian, eight were cured and four not cured; of the ten cases of quartan, six were cured and four were not cured. On the whole, eighteen were cured and fifteen not cured. He found relapses frequent. The patients did not recover so quickly or so well as under the influence of quinine. Nevertheless, he is inclined to think that it should be retained in the list of febrifuges, since it is much cheaper than quinine, and may therefore be employed by a poor population with advantage. The experience of his *confrères* was very similar to his own. (*Bulletin Général de Thérapeutique*, No. 12, 1873.)

**Diagnosis of Ovarian Tumours.**—A translation of an essay on this subject, by Prof. Otto Spiegelberg, appears in the last number of the *American Journal of Obstetrics*. He does not detail the different signs of ovarian tumours, but follows the course which the physician at the bedside has to take. Suppose, he says, a patient comes who, from enlargement of the abdomen, with abnormal resistance and various unusual symptoms, believes herself to have an ovarian tumour. The first task is to determine whether a tumour exists at all. An affirmative answer is easily arrived at, where there is a well-defined mass distinctly separated from the neighbouring parts. It is very different, however, when the contour is not distinguishable, and palpation is fruitless on account of tenderness



and the tension of the abdominal walls. Here there may be a cyst, but by no means necessarily, for there are two conditions in particular which on superficial examination so resemble a cyst that they are not rarely mistaken for it, viz., tympanitis and ascites. The presence of tympanitis is proved by the absence of fluctuation and the results of percussion, which will give resonance everywhere in place of the dulness which any fluid collection, whether free or encysted, would cause. But beyond this we have a sovereign means of diagnosis by which the condition of things may be demonstrated to the eye, in chloroform narcosis, as recommended by Simpson. In this state the muscles relax, the abdomen collapses, so that deep palpation becomes possible. The same result may be more quickly reached by placing the patient on her back, with knees drawn up, and telling her to breathe naturally. The two hands, laid flat on the abdomen, may then be pressed firmly towards the spine. The walls yielding a little with each expiration, what is gained must be retained through the pause and the next inspiration. After several repetitions of this manœuvre, all opposition is overcome, and the hand can thoroughly palpate the abdominal and pelvic cavities through the now flaccid walls. Masses of fat have occasionally been taken for ovarian tumours, especially at the climacteric age. Such masses are elastic, with indefinite outline, and they are dull on percussion. Repeated bimanual examination of the internal genital organs, deep percussion after proper emptying of the intestine, attention to the presence of a general development of fat rarely present in cases of abnormal growths, the possibility of lifting the curtain of fat from the subjacent parts, the comparative thinness of the inguinal and lumbar region where the fat is always less abundant, will usually suffice to make these cases clear.

The mistaking of ascites for cyst is possible only when fluctuation has shown the presence of fluid, and the distinction is easy when the filled and tense sac can be grasped and moved backwards and forwards under the abnormal walls; and conversely when the fluid, being nowhere sharply bounded, can be felt to slop about in its lower portions, and to change its level with every change of position of the patient. Difficulty occurs only when the collection has become extensive. In ascites the abdomen is equally prominent on the two sides; the lumbar regions are distended, the umbilical relatively flat, whilst the umbilicus itself is frequently prominent. With cyst, on the other hand, the abdomen is more conical in form, or at least cask-shaped; the umbilicus is relatively higher, and is not unduly prominent; not unfrequently the distension of the abdomen is unequal, one side being more prominent than

another. There is nothing distinctive in enlargement of the veins. Oedema of the lower extremities, though more frequent in ascites, is a symptom of no real diagnostic value. The most important distinctions are afforded by palpation and percussion. In ascites the fluid gravitates in every position towards the most dependent part, and the area of dulness on percussion changes with the position of the patient; whilst with encysted fluid the territories of resonance and dulness remain absolutely or approximating the same, no matter what position the woman takes up. In the dorsal decubitus, then, supposing the fluid to be free, the lumbar region will be dull and the umbilicus resonant, from the bowels floating on the surface of the fluid; and the converse will be true in the case of the cyst, since those cysts which rise above the brim of the pelvis lie against the anterior abdominal wall, crowding the intestines upwards and backwards. Still there are various points which may easily deceive. First, it is by no means unfrequent in ascites to find the right iliac and even lumbar region tympanitic, on account of the presence of the cæcum, which is often greatly distended with gas, and is confined in its position by a short mesentery. Again, we now and then find a fold of intestine lying in front of the lower anterior border of the tumour, especially when it is narrow below; and this causes a dull tympanitic tone in the hypogastrium. Fluctuation may under favourable circumstances be very distinct in a cyst, or in fat people may be indistinct in ascites. Here a point of diagnosis may be made use of, to which the late Professor Breslau, of Zurich, first called attention, viz., in a cyst, fluctuation ceases at the point where percussion becomes tympanitic (lumbar and epigastric regions), the sac wall affording a complete separation between them; but if the fluid is free in the cavity, if it surrounds the intestines as in ascites, the fluctuation will be felt, even in parts where the intestine gives a tympanitic sound. In great distension of the abdomen, either by ascites or by a cyst, all these signs may fail, and puncture can alone reveal the truth. Dr. Spiegelberg places the greatest reliance on exploratory puncture as a means of diagnosis in difficult cases. The contents of ovarian cysts vary from a watery, clear, yellowish fluid, to a tough colloid, stringy, dirty-brown or yellowish-green mass, containing mucin, albumen, and paralbumen, and in addition *cylinder epithelium* in various states of change, débris of cells, crystals of cholesterin, and now and then red blood-corpuscles and pigment granules. Ascitic fluid is always thin and comparatively clear, is poor in solid constituents, and as a rule deposits a copious delicate coagulum of fibrine. In addition it contains the endothelium of the peritoneum, and wandering cells derived from the latter.

If all these means fail in enabling a diagnosis to be made, Simon's plan must be adopted, which consists in introducing the half or the whole hand into the rectum, though it should be explained to the patient that she must be deeply narcotised, and that there will be some slight lacerations of the anus; that she will have to keep quiet for some days, and that she may have trouble in defæcation for ten or twelve days. Finally, an exploratory incision may be made after carefully emptying the bladder and rectum. (Translation of No. 55 of the *Klinische Vorträge*, by L. Wheeler, in the *American Journal of Obstetrics*, vol. vi. No. 3, 1873.)

**The Etiology and Therapeutics of Cancer Uteri.**—Dr. Edward Martin, from observations made in the Berlin Gynæcological clinic, found that in 87 out of 93 post-mortem examinations the cancer proceeded from the vaginal or cervical portion of the uterus, and was only in six cases limited to the body of the uterus. Examinations made during life frequently demonstrated that the origin of the new formation occurred in the fold of the posterior vaginal arch, close to the neck of the uterus, in the form of small knots and granulations. In three cases the origin of the cancer appeared in the form of small red granulated condyloma, like flat outgrowths at the border of the anterior or posterior lip of the os. In one of these cases he recommended the patient to have the part excised, but she delayed too long, and died from fully developed cancer uteri. In the two other cases he removed the diseased part with the cerasur, and then applied Vienna paste freely; both these patients recovered completely. Microscopical examination of the tumours showed that they were of cancerous nature. The post-mortem examination showed that in many cases secondary cancerous deposits had taken place in the liver, pleura, peritoneum, lungs, kidneys, thyroid gland, bones, stomach, and brain. Hereditary influence does *not*, he thinks, play so important a part as is commonly attributed to it in the development of cancer. He only obtained certain evidence of cancer in the family in 13 out of 65 cases. He is not able to corroborate the statement made by certain French writers that early menstruation predisposes to cancer, nor does he find it true that very frequent delivery favours its occurrence. He thinks, however, there can be no doubt that intercourse with men suffering from syphilis or its consequences has a tendency to produce it. Widows were often the victims of it, though their husbands may not have shown other signs of syphilis or gonorrhœa than stricture; and not unfrequently the cancer developed on second marriage. Cancer is well known to be a disease more common in great towns than in small, or in the open country. In reference to

treatment, it is important to distinguish between cancer uteri and the granular erosions of endometritis colli, as well as certain chronic inflammations of the uterus after abortion, in which the vaginal wall is thickened, infiltrated, dense, and the lips adherent to one another. In these conditions, attention and proper treatment by cauterisation, &c., may often bring about a cure. In true cancer uteri removal can only produce a cure when the growth is very small and limited to one lip. The section must take place through healthy parts, and after several days' delay the wound must be freely touched with Vienna paste. Recently the use of the knife, scissors, guillotine, and of the ecraseur, with the use of the sharp spoon, have been much vaunted, but it frequently returns after such operations, as it does after the application of the actual cautery, the gas-burner, or the galvano-caustic instrument. The destruction of the growth by chemical means, such as caustic potash, Vienna paste, hydrochloric acid, or chloride of zinc, subdues for a time the hæmorrhages and the pain, but do not prevent relapses. Injections of nitrate of silver or bromine into the tumour are useless. In regard to palliative treatment, the watery mucous secretions are best treated by injections of solutions of sulphate of alum or copper; against hæmorrhage, injections of cold water with solution of tannin or with diluted liquor ferri may be employed, or tannin in powder may be scattered over it, or aqua creasoti thrown in, or plugging with lint dipped in solution of tannin used; and when there is much stench, creasote-water, permanganate of potash in solution, and chlorine may be applied. Chlorate of potash, in solutions containing one part in 16, is very serviceable. To relieve the pain, morphia or opium or chloral must be given. A not unfrequent complication, hydronephrosis of one or both sides, causing considerable diminution in the flow of urine, and secondary complications, he treats with small doses of iodide of potassium. In thromboses of the femoral vein, he applies cloths wetted with lead lotion and woollen covering. Peritonitis he treats with cold or hot poultices. (*Berliner Wochenschrift*, 28, 1873, and *Der Praktische Artz*, No. 10, 1873.)

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[A copious French Bibliography will be given in our next number.]

<sup>1</sup> Any of the foreign works may be procured on application to Messrs. Dulau, of Soho Square, W.C. ; Williams and Norgate, of Henrietta Street, Covent Garden, W.C. ; or Baillière, of King William Street, Charing Cross.



## Department of Public Health.

### SPIRITUS ETHERIS NITROSI.

WE had not intended to carry any further the discussion about this drug, and about the failure of the recent prosecution, by the Westminster Board of Works, of druggists charged with selling adulterated specimens of it; but a leading article which appears in our contemporary the *Pharmaceutical Journal* compels us to take further notice of this unpleasant subject. The writer of that article—and we sincerely hope he is not, this time, the true representative of the Pharmaceutical Society—appears to be intoxicated with the success of the accused druggists whom he so warmly supports; and in the excitement of his immoderate joy allows his real hopes and intentions to appear. These hopes and intentions tend, as it would seem, to the object of “driving a coach and four” through the Adulteration Acts whenever their enforcement shall threaten a considerable interest which the drug-trade may possess at the time. We will not at present believe that the respectable men who form the council of the Pharmaceutical Society have really issued this imprudent article as a manifesto of their intentions. But it is not the less necessary to show, at once, that such action on the part of the trade would recoil severely on the heads of those who counselled it.

The *Pharmaceutical Journal* takes up Dr. Stevenson’s article (in the January *Practitioner*) upon the difficulties which lie in the path of the public analysts, and the unjust spirit of detraction with which their efforts have been met. We have not space to deal with the whole of the arguments by which the writer seeks to throw discredit on the attempts of analysts to pin tradesmen

down to a definite standard of purity in the articles they sell. But the manner in which he speaks of Dr. Dupré, and of the "false position" in which he represents that gentleman to have placed himself by instigating the prosecution for adulteration of spir. etheris nitrosi, is not to be tolerated. The view of the facts of that prosecution which the trade has unhappily chosen to assume, and on the strength of which the *Journal* presumes to lecture Dr. Dupré, is demonstrably incorrect in every particular. We were from the first more than half persuaded that this must be the case, and we have now inquired afresh into the matter, with results which are not a little surprising when we remember the energy with which the imputation of adulteration has been repudiated.

1. Fallacy No. 1 which the *Journal* adopts, and which the magistrate, Mr. Arnold, to our astonishment endorsed, is that the accused article was not a medicinal, but only a popular, preparation. There might be some, though not much, force in this view were it true (as the trade journals represent) that Dr. Dupré's inspectors asked for and purchased "sweet spirits of nitre." It is a fact that one of them did so, by mistake; but the other two handed in written orders for "spir. eth. nitros," thereby making it plain that they required the pharmacopœial article; and the summonses were sent out for adulterating "spiritus etheris nitrosi." The following table gives the results of analysis. No. 1 is the British Pharmacopœia preparation properly made; Nos. 2, 3, 4, and 5 are the incriminated samples:—

TABLE I.

	No. 1.	No. 2.	No. 3.	No. 4.	No. 5.
Specific gravity .	·845	·867	·849	·928	·851
Corresponding alcoholic strength	81·2 p. ct.	71·9 p. ct.	79·5 p. ct.	45·7 p. ct.	78·7 p. ct.
Real nitrous ether	about 3 p. ct.	0·36 "	0·57 "	0·25 "	0·07 "
Free volatile acid, as acetic acid .	none	0·70 "	0·54 "	0·30 "	0·26 "
Free fixed acid .	none	trace	trace	trace	trace

Nos. 3 and 4 were sold in answer to a written requisition for Spiritus etheris nitrosi.

We shall confine our attention to samples 3 and 4. These were asked for as "spir. etheris nitrosi," and the druggist was

therefore unquestionably guilty of a fraud if he *knowingly* supplied anything but the preparation ordered in the British Pharmacopœia. We use the same old-fashioned phrase which was employed by Pereira, who years ago denounced an exactly similar falsification which he declared was common in the trade. Of course the excuse is open that the retail trader knew nothing of the matter, but trusted in the wholesale chemist from whom he purchased the drug. Does anyone seriously believe that the wholesale chemist was guilty of a mere *mistake* when he sent out, under the name of "spir. eth. nitrosi," such an article as either of those described in columns 3 and 4 of the above table?

2. The next fallacy which the trade party propounded in the police-court and have repeated elsewhere is, that it could be no object to weaken the preparation by substituting alcohol for nitrous ether, seeing that the former was *actually cheaper than the latter*. We think that this is about as cool a statement as any that has come under our notice. It is a pity that Dr. Pereira was not alive to hear it: for in vol. ii. part 2 of his great work (p. 1,973) he expressly adduces the fact of the cheap price at which so-called spir. ether. nit. was sold, as plain proof that the "hyponitrous ether" was replaced by water or spirit and water. It certainly requires a good deal of assurance to declare, in open court, that an article manufactured from alcohol costs less than alcohol itself!

3. The worthy magistrate, however, not only swallowed this marvellous statement, but volunteered an argument, on his own part, which is a delicious specimen of magisterial logic. The defendants had attempted to ride off upon the suggestion that Dr. Dupré wanted to compel everybody to make spir. eth. nit. by the process of the 1867 Pharmacopœia. The suggestion was perfectly untrue, for Dr. Dupré's only complaint was that the incriminated specimens did not come anywhere near the percentage of nitrous ether (surely moderate enough) which is required by the British Pharmacopœia, and were consequently useless. But Dr. Dupré did incidentally mention that by employing copper, as in the new process, it was easy to make an article of the required strength. Whereupon the magistrate said that it would be most unjust to call a man an "adul-

terator" who merely left out one ingredient in the article he was making! The worthy man seems to have forgotten that this argument might be pushed a step further. Why should not the manufacturer leave out the alcohol? It is true that in that case he would be selling nitric acid instead of sp. eth. nit.: but what is that to Mr. Arnold?

4. Another fallacy which has been extensively circulated is, that it is not possible to make *and keep* sp. eth. nit. at a uniform strength. Dr. Dupré has supplied us with the following facts, which completely contradict this statement. He personally prepared a sample of sp. eth. nit. according to the pharmacopœial process, on Feb. 21, 1872, and stored it in an ordinary glass stoppered bottle, which it only half filled. Analysed at the time of its manufacture, the article gave the results recorded in column 2, Table II. The bottle was repeatedly opened to examine the smell and acidity, and on Nov. 15, 1873 (twenty months after the manufacture), the sample was again analysed, with results that are given in column 3, Table II. It will be seen that although the amount of free acid was now considerable, owing, no doubt, chiefly to the oxidation of aldehyde, the amount of real nitrous ether present had scarcely altered during twenty months of by no means careful stowage:—

TABLE II.

	No. 1. <sup>1</sup>	No. 2.	No. 3.
Specific gravity . . . . .	·824	·836	·839
Corresponding alcoholic strength	89·5 per ct.	84·8 per ct.	83·6 per ct.
Real nitrous ether . . . . .	1·35 „	2·00 „	1·98 „
Free volatile acid, as acetic acid.	1·19 „	none	0·9 „
Free fixed acid . . . . .	trace	none	none

5. The first column in Table II. affords proof that the adulteration of sp. eth. nit. at the fountain head is no figment, but a serious fact. It represents the analysis of a sample obtained in the following manner. After the trial in the Westminster Police Court Dr. Dupré determined to examine the sp. eth. nit. of some wholesale firm of repute; accordingly he purchased directly, at a very famous wholesale establishment, *an article labelled*

<sup>1</sup> This sample contained a considerable amount of some organic chloride, probably chloride of ethylene, showing that very impure materials must have been used.

"*Spiritus Etheris Nitrosi Brit. Pharm.*" As may be seen in the table, it was not merely seriously deficient in nitrous ether, but otherwise obviously impure, and *carelessly made*. It is clear as daylight, then, that the cry that "uniformity of standard in spir. eth. nit. is impossible," is one of dubious sincerity. Meanwhile the abuse is grave. Here was a high-priced article (3s. 9d. a pound), from a first-rate house, which sinned in every direction.

On a review of the whole proceedings in the police-court, it strikes us that Westminster Board of Works must have been feebly represented by their lawyer, or such obvious fallacies as were allowed to pass current must have been refuted on the spot.

We have written the above analysis of the merits of the sp. eth. nit. case, not from the slightest unfriendly feeling to the trade, but because we cannot sit still under a grave misrepresentation of facts which is not only most unjust to a very able and high-minded chemist, but tends unquestionably to throw serious obstacles in the way of the working of the Adulteration Acts. We would entreat the members of the trade, for many of whom we have the highest esteem, to deal frankly with this subject; and we would beg the editor of the *Pharmaceutical Journal* to seriously reconsider his position. What can he mean by attempting to throw doubt on Dr. Stevenson's allegation that "shameful" irregularity exists in the strength of citrate of quinine and iron as sold in the shops? *No man knows better than the Editor of the Pharmaceutical Journal* that the *Practitioner* long since demonstrated the fact of an extreme laxity in the manufacture of this article; that the publication of our analyses caused no small disquietude at the time; but that the fact which they disclosed has over and over again been confessed by several reforming members of the Pharmaceutical Society since that time. Our contemporary will see that he only exposes himself to ridicule when he affects to doubt a matter which is quite notorious.

One word in conclusion. It has been urged to us in private conversation, by a member of the drug trade for whom we have a very high respect, that although it might be easy to make a *small* sample of spir. eth. nit. which should be stable at a correct



strength, it might not be so easy, if at all possible, to ensure the same result in large operations. But on careful reflection and inquiry we find no ground for believing that there is any difficulty of the sort if the process of the British Pharmacopœia be followed.

## ON THE INFLUENCE OF AGGREGATION ON THE SICK IN HOSPITALS FOR INFECTIOUS DISEASES.

MORE lasting than any political results of the Crimean war, have proved the advances in hospital construction and administration made in consequence of the disastrous events of the early part of the campaign. It is questionable whether the lessons taught at that time have been carried into practice as fully as they deserve. Hospital huts might with advantage replace for some purposes the palatial buildings which are still erected. Experience has shown that the healthiness of an hospital decreases with increasing age. If external appearance were less studied than it is, it would be possible, at very little if any additional cost, to provide a succession of buildings possessing every requisite internally that the best hospitals require. These renewed every ten or twelve years would not acquire the hospital taint, and would never become antiquated. The Americans, from the experience of the War of Secession, have adopted on an extensive scale the use of wooden huts as hospitals for epidemic diseases. At the present time, when the erection of special hospitals for the treatment of epidemic and contagious diseases is being extensively carried out in this country, the question of the results obtained in this class of hospitals becomes one of great interest. In a late number of the *Gazette Hebdomadaire*, M. Léon Colin has published an able article on the subject, in which it is treated entirely by the light of French experience. It furnishes evidence that on many points connected with hospital provision for outbreaks of epidemic disease our continental neighbours lag behind us. It has also the disadvantage, from the absence of a precise statement of the amount of cubic space and other hospital requirements considered by the writer as absolutely essential, of seem-

ing in certain diseases to excuse some amount of overcrowding, a retrograde step we cannot think it is meant to countenance. But on the whole the paper is a most interesting one, dealing with the effects produced by the size and kind of hospital on the patients who are treated therein—in short, on the question of “hospitalism” as it affects contagious diseases. As the diseases to be treated are very various in their causes and character, so, it is argued, it is impossible to lay down fixed and invariable rules for the construction of hospitals intended it may be for the treatment of diseases as different as small-pox, typhus, and cholera. The principle of construction is essentially a question of medical science, and can only be dealt with by men who know the physiology of disease as well as of health, and who are intimately acquainted with the natural history of the disease to be treated. Architects have a tendency to reproduce fixed types and to perpetuate the one form of hospital building which they consider nearest perfection, without sufficient consideration of attendant circumstances. Only where diseases and their causes become things of mathematical precision can their treatment or prevention be carried out by inflexible rule. It can scarcely be expected that in the intervals of epidemics permanent hospital space will be provided sufficient to accommodate all the sufferers from an outbreak: it becomes then a question of importance, and one which the Medical Officer of Health may be hurriedly called upon to decide, What kind of temporary accommodation is suitable for the special disease? Whether existing buildings can be safely utilised, or whether the erection of huts or the treatment of the patients in tents ought to be recommended, becomes then a point upon which the possession of clear ideas is of much consequence. Taking several of the chief epidemic diseases, M. Colin discusses at more or less length each of them from this point of view. The first is small-pox. With reference to this disease it is stated that ascertained facts “establish the freedom from injury to each other of small-pox patients collected even in considerable numbers in the same institution,” and that “during the siege of Paris the treatment within a few months of nearly 8,000 small-pox patients in a single establishment, of which the wards were connected with each other, was

not productive of danger either to the patients themselves or to the hospital staff or to the neighbouring population." This is said to be a proof "that the result in individual cases depends more upon the energy with which the contagious germ attacks the system than upon the surrounding circumstances during treatment." These conclusions, given without the data upon which they are founded, cannot be received without qualification. It is well known that the percentage of mortality was considerably higher during the late epidemic in Paris than in London, but without the statistics relating to the state of vaccination in the respective cities it is impossible to institute an exact comparison. The experience of the Hampstead Hospital, in which about 7,000 patients were treated during the late epidemic, showed that a very large number might be aggregated in one place without injurious effects when under favourable circumstances. The style of building (single storied pavilions, with ample provision for ventilation), an allowance of close upon 2,000 cubic feet of space to each patient, an open and airy site, reduced the risks of "hospitalism" to a minimum. Admitting that the mere aggregation of a large number of small-pox patients in one building will not intensify the disease in itself, it must be borne in mind that a ward filled with severe cases of small-pox ceases to be a purely medical ward, and becomes to a great extent surgical and subject to the laws applicable to that class of diseases. M. Colin admits to the fullest extent the danger attending the aggregation of patients suffering from suppurating wounds; like dangers attend the aggregation of small-pox cases. Theoretically this is to be expected, and experience confirms the theory. Overcrowding or deficiency in the hygienic condition in small-pox is followed, as in surgical diseases, by erysipelas, gangrene, and pyæmia. The amount of cubic space recommended by the Medical Department of the Local Government Board, namely 2,000 feet to each patient, cannot without risk be reduced. If this amount is not to be obtained in permanent erections, it is better to treat the patients in huts, or, if the season permit it, in tents.

For scarlatina, M. Colin claims, but with a great degree of doubt, "a certain independence between the extension and the severity of the disease on the one part, and the hospital condi-

tions imposed upon the patients on the other." He advances as proofs of this independence the observations made in England showing the relative frequency of scarlet fever in elevated, airy, and seemingly healthy localities, and also the fact that in our climate this disease generally prevails in summer, the season when free ventilation is most comfortably and generally carried out. Measles, it is asserted, is a disease in which the power of the virus itself is not so generally dangerous as in small-pox or scarlet fever. In measles, bronchial complications give to certain epidemics an exceptional severity, and these complications owe their existence to certain meteorological conditions. Facts are adduced intended to show that aggregation in the wards of hospitals sometimes increases the fatality of the disease. One striking instance is quoted, where, at Val de Grace, crowded by the return of the army from Italy, the deaths from this disease were the enormous number of 40 in 125 patients, a mortality twelve times greater than the average of the army generally, which was 3 per cent. of the sick. M. Laveran, the physician in charge, ascribes this, not to the influence of the measles patients on each other, but to the general unhealthiness of the place at the time, owing to an accumulation of a number of wounded soldiers and sufferers from dysentery. This supplies a proof, not so much of the danger of treating measles in special hospitals, as of the intense aggravation of the disease by overcrowding, and the great necessity there exists in this, as well as in other epidemic fevers, of free aëration. English experience of the treatment of this disease in hospital has hitherto been very limited. It is not likely to become much more extended if the practice adopted in London be carried out, as patients suffering from the disease are not received in the metropolitan fever hospitals. In the case of scarlet fever, also, the facts relating to the effects of aggregation in hospitals in England are not sufficient to found a decided opinion upon, but the advantage of free ventilation of the wards in the treatment is generally accepted.

There is one infectious disease which, whether we admit that it owes its origin to overcrowding or not, undoubtedly is fostered and spread by its agency, namely, typhus; and with regard to it M. Colin says "the severity is always in exact mathematical relation to the number of sick collected in the



same establishment," and quotes an example from Crimean experience, in which one ambulance out of a population of 375 typhus patients lost 370. The danger of aggregation of typhus patients is very generally admitted, and can only be averted in permanent buildings by the freest possible ventilation of the wards. Typhus cases will do well when exposed to currents of cold air, which in some diseases, as measles for example, might prove exceedingly dangerous. There is a class of infectious disorders in which it is impossible to overrate the fatal effects of aggregation. It consists of those diseases which, as far as we know, originate in hospital miasma itself, such as gangrene and puerperal fever. In their treatment only one course can be adopted with safety, and that is to isolate each case individually.

We come now to the consideration of the question as far as it relates to cholera. From the character of the outbreaks, in cholera more than in any other epidemic disease, it is likely that hospital accommodation will require to be suddenly extemporised, and in none is isolation of the sufferers more useful. For three reasons, which seem to us to have considerable weight, M. Colin believes that the aggregation in hospitals of persons suffering from cholera is harmless to themselves. These reasons are, first, that in cholera epidemics the mortality is as great amongst those treated at home as in those removed to hospitals; second, that small hospitals possess as high a rate of mortality as the large do; and third, that the worst epidemics of cholera, differing in this respect very much from those of typhus, have not had their *origin* amongst crowded town populations, but in caravans, or in an army in the field, sleeping in tents or in the open air, and that in this respect cholera approaches more nearly to diseases depending upon the influence of the soil than to those which result from overcrowding. Into the question raised in the last reason we do not enter here. In Germany, in the late, or, to speak more correctly, the present epidemic, removal to hospital has not been found to lessen the chances of recovery to the patient, and English experience in former epidemics accords with this view.

What is the best form of temporary hospital for the treatment of cholera patients? is a question to which at any moment it may become of urgent importance to give a decided answer.



In favour of tents, the readiness of erection and the possibility of obtaining without trouble complete isolation of site may be justly urged ; on the other hand, the danger of impregnating the soil with the poisonous discharges of the patients is obviously greater in tents than in any other form of hospital, although the same objection applies in a lesser degree to huts if loosely constructed. Again, it is found that a certain amount of external warmth is required to assist the cholera patient in rallying from the stage of collapse ; and in the case of tents there is a difficulty in this climate, at some seasons, in obtaining the necessary temperature. In the late small-pox epidemic accidents occurred which proved the dangers attending the use of stoves in tents. During the epidemic of cholera at Gallipoli and Varna, in the time of the Crimean war, a decided lessening of the disease was noticed when the sufferers, up till that time collected in hospitals, were removed into well-ventilated tents. This improvement can easily be understood when it is considered that the hospitals were old Turkish buildings, dilapidated, without sewers, and totally destitute of means of ventilation. It cannot therefore be taken as a decided proof of the superiority of tents to properly-constructed hospitals for the reception of cholera patients. In conclusion, it may be accepted as proved that epidemic diseases vary in their tolerance of hospital aggregation, but that in all overcrowding is followed by disastrous results.

The form of hospital employed ought to vary with the disease to be treated. It is scarcely to be expected, or indeed necessary, that distinct special permanent hospitals are to be erected for each form of contagious disorder, but when providing temporary accommodation during an outbreak, it is well to bear strongly in mind the peculiar requirements of the disease to be treated. As a general principle it may be assumed that in the case of cholera, small-pox, scarlet fever, measles, and, let us add, typhoid, buildings already in existence which possess the necessary requirements of isolated site, ample means of ventilation, and space sufficient to allow at least 2,000 cubic feet to each patient, may be safely utilised ; in some diseases, such as typhus, it is better at once to proceed with the erection of hospital huts to receive the patients.

## PARADOXICAL ETIOLOGY.

1. *Mr. Wanklyn on "Milk Panics."*<sup>1</sup>
2. *Dr. Hardwicke "On the Alleged Outbreak of Typhoid in Marylebone."*<sup>2</sup>

IF it were not that the title would suggest awkward comparisons, we would forthwith, after the fashion of the late Augustus de Morgan, establish a *budget of paradoxes*. Paradoxers, to use De Morgan's term for them, begin to swarm, and we have need of some such relief as may be obtained by putting them together, "so as to enable those who may have been puzzled by one or two of them to see how they may work in the lump." "A paradox," writes De Morgan, "is something which is apart from general opinion, either in subject-matter, method, or conclusion. Many of the things brought forward would now be called *crotchets*. But there is this difference, that by calling a thing a *crochet* we mean to speak lightly of it; which is not the necessary sense of *paradox*." Dr. Cunningham's Report on Cholera in Northern India in 1872, which we recently noticed (*Quixotic Etiology*, Dec. 1873), would be best described as a *paradox*; the two works we are about to notice, as *crotchets*.

1. Mr. Wanklyn's observations on milk panics relate to some facts in connection with what he terms the milk panic of 1873, and he arrives at the comforting conclusion, first, that there has been no typhoid fever in connection with the milk supply of a certain Dairy Company in Marylebone; and secondly, that the district supposed to be affected has seldom been so free from typhoid fever as during the period of the supposed epidemic.

Incidentally he gives us some interesting information respecting the etiology of panics. A panic may, it appears, like many infectious diseases, be traced back to a single first cause, and

<sup>1</sup> "Milk Analysis: a Practical Treatise on the Examination of Milk and its Derivatives—Cream, Butter, and Cheese." By J. Alfred Wanklyn, M.R.C.S., Public Analyst for Bucks, &c. Chap. xv.: "Poisonous Milk and Milk Panics." Sm. 8vo. pp. 70. Trübner, 1871.

<sup>2</sup> Read before the Social Science Congress, Oct. 1873. *Fly-sheet*.

may, like those diseases, at first spread among those persons in constant communication with that first cause ; afterwards, when the moral atmosphere becomes highly charged with contagion, the disease may assume epidemic proportions and attack a large number of those persons within range of its influence. The incubation period in panics appear to be remarkably short ; in the present instance, a single case in a few days produced an alarming epidemic.

From Mr. Wanklyn's account it appears that this milk panic originated with a certain " eminent West-end physician," several of whose children were ill with typhoid fever early in August, and spread in the following manner. The doctor, we are told, " attributed the disease to the milk which they took." On communicating his suspicions to neighbouring medical men, and to the Medical Officer of Health for the district, a number of cases of alleged typhoid were found among other customers of the dairy supplying the doctor's family ; " a strangely large proportion of cases occurring in the families of medical men." It is odd that the mental obliquity which attributed the disease to milk-poisoning should, like the fever, have attached itself to the medical profession ; but such seems to have been the case, for neighbouring medical men and the Medical Officer of Health took this mental affection of the physician : together these persons appear to have tried (no doubt experimentally) to infect the directors of the Milk Company as well as the Local Government Board. The directors of the Milk Company were, it appeared, " protected " against infection of this kind : not so the Local Government Board ; the latter—possibly owing to its possessing a Medical Department—became infected, and " an official investigation was ordered." About this time the press, " non-technical," sadly enough, as well as medical, caught the infection and took up the subject, the " leading journal " to the extent of publishing " a list of twenty-three households wherein inmates were said to have been poisoned by the Milk Company ;" and the panic was at its height. It is satisfactory to note that one medical man was proof against mental infection. Mr. Wanklyn, in the dual capacity of analyst and doctor, " took the trouble to inquire into some of these cases." He visited four of the twenty-three households said to be infected. " In one of these households there was no one

ill, and there had been no one ill. In another household, there had been only a little summer diarrhoea. In a third, the lady had been taken ill at Munich, where typhoid fever is known to be rife. In a fourth, where the servants were affected, the water in the kitchen was bad, the general supply to the house being good. *The servant* had, moreover, been a day's journey into the country during the very hot weather, and had been overheated." Having found illness in three out of the four households visited, Mr. Wanklyn "did not pursue the investigation further." This is unfortunate, for the inquiry promised to be interesting, and the facts elicited, so far as they went, might, by reason of the peculiar mental bias already described, have been thought to show that there was reason for suspecting the presence of typhoid fever in some of the households above mentioned. Why was the inquiry dropped suddenly at this point? Can it be possible that Mr. Wanklyn had some misgivings as to the analytic element in his composition possessing the power of neutralising the natural susceptibility of his medical organism to contagion? In other words, may not Mr. Wanklyn have found himself likely to be placed in the dilemma of having to deny as a chemist what he was unable to disprove as a medical man? This suspicion once aroused, receives some confirmation from his remarks on the subject of water-poisoning.

He tells us that "a very subtle kind of poisoning has been described. It has been said that if a very minute quantity of water from a foul well be mixed with a very large quantity of milk, the whole mass of milk will become poisonous." As an analyst, Mr. Wanklyn appears to ridicule any such possibility, and it is doubtless a fact that, chemically, water from a foul well is not to be distinguished from specifically infected water by any known process. As a medical man, however, he, while discussing the reported existence of a case of typhoid fever at a farm from which the London Milk Company was in part supplied, seems to admit that the addition of specifically infected water to milk might be attended with consequences dangerous to persons drinking it; he even appears to be possessed of data respecting the time required, in a particular case, for the passage of specific excreta into well-water, for he tells us that "on the supposition of infection from one of the farms," he would—for

the production of an epidemic in the beginning of August—"hardly look for a case at the farm before the beginning of July;" indeed, he considers a case in early June too far back to have had any influence in producing an epidemic in the beginning of August. It is to be regretted that Mr. Wanklyn has not given the data upon which this opinion is founded.

Finally, "Marylebone has seldom been so free from typhoid fever as during the period of the supposed epidemic." In support of this statement Mr. Wanklyn quotes the returns of the Registrar-General, and gives in a tabular form the weekly deaths from typhoid fever in Marylebone during July, August, and part of September, 1873. His table is reproduced below, with the addition of the mortality from typhoid fever in the same district for the corresponding weeks of 1871 and 1872.

DEATHS FROM TYPHOID FEVER IN MARYLEBONE DURING—

	1871.	1872.	1873.
27th week . . . . .	1	0	0
28th „ . . . . .	0	0	1
29th „ . . . . .	1	0	2
30th „ . . . . .	1	0	0
31st „ . . . . .	1	1	1
32nd „ . . . . .	1	1	1
33rd „ . . . . .	1	1	3
34th „ . . . . .	0	0	2
35th „ . . . . .	0	2	2
36th „ . . . . .	1	0	2
Total during ten weeks . . .	7	5	14

Mr. Wanklyn is a better chemist than arithmetician.

2. Dr. Hardwicke is the Medical Officer of Health for Paddington, and he is obviously a social as well as scientific paradoxer. He has no hesitation in questioning publicly the facts averred, as to the prevalence of typhoid fever in Marylebone, last summer by his brother medical officers of Marylebone and St. George's, apparently upon no other grounds than the reports which appeared in the daily and other journals at the time. This is the social paradox. Is a Medical Officer



of Health, by reason of his position, exempted from the comity of manners which holds good in the ordinary practice of medicine?

As to "typhoid-poisoned milk," Dr. Hardwicke thinks that "there are many scientific reasons which render this theory extremely doubtful," and he regrets that "in a matter of such vital interest hasty conclusions should have been arrived at by some medical authorities, especially as the statements put forward (and which caused such unnecessary alarm) have not been borne out by subsequent investigation." The suggestion that in the Marylebone outbreak (assuming the facts to be as stated) the milk accused of the mischief had been poisoned by a few drops of water possibly contaminated by the diarrhœal discharges of a typhoid fever case, he sets aside on two grounds; to wit, first, that there is, he holds, no evidence of the existence of such a case, and next, that he has "serious doubts of the possibility of a few drops of water poisoning so large a quantity of milk, especially when the extreme dilution is considered, the hypothesis being in direct opposition to scientific results obtained by recent investigation." To this point the argument is pretty straightforward. Now comes the paradox. Having disabused his readers of the notion that there is any sufficient reason for the belief in outbreaks of typhoid fever from typhoid-poisoned milk, he next proceeds to show that pollution of milk by typhoid-poison is not only possible, but highly probable; and that the danger of this pollution is much more considerable than even the authorities whose "hasty conclusions" he condemns had supposed. Instances have come under his observation which prove that milk, as well as water, can absorb sewer air. "In a dairy where milk was put to stand for cream," he writes, "some carbolic was used to disinfect its drain, and the volatile vapour of the acid being absorbed by the milk, the man's customers complained that the milk was so poisoned by the carbolic that it was unfit to be mixed with tea or used for any other purpose. I therefore have evidence that cream rapidly absorbs sewer gas of our drains, as we already know water does, and that this dangerous element has been overlooked in this instance." Now, the state of things in the parts of Marylebone where the fever is said

to have prevailed, and also in Paddington where cases occurred, is described by Dr. Hardwicke as follows :—"Untrapped drains, soil-pipe of water closet poisoning the cistern through the waste pipe, water consequently dangerous to drink, bad ventilation, in fact all the necessary conditions under which fever might be expected to occur, filthy emanations also arising, which alone would pollute the air breathed, the water or milk drunk, or the food eaten by persons living in such houses." We commend this description to the notice of Dr. Hardwicke's Vestry. The rest of the argument must be given in Dr. Hardwicke's own words :—

"Under these circumstances, it does appear extraordinary to me that the cause should be looked for in so remote a locality as forty miles away from the seat of the mischief. Knowing well the state of farmyard arrangements and cow-sheds in country villages throughout the kingdom, I am not surprised to find that well water was polluted on the particular farm alluded to, as many thousands are polluted in a similar manner at the present moment, but—[and here follows Dr. Hardwicke's doubts as to the possibility of milk in large quantities being polluted by a few drops of water]. The suspected dairy, moreover, is situated in a district where bad drains exist, and where at the critical moment structural arrangements were going on in the immediate vicinity, so that foul gas might have found its way into the premises and so poisoned the milk; which would corroborate my previous experience that foul air will contaminate both milk and water, and so cause enteric fever; and that it is a much more probable cause of the mischief than the apocryphal case placed before the public by those who are responsible for the propagation of the panic. It is also a significant circumstance with regard to this inquiry that no professional examination was made of all the cows yielding milk for the suspected dairy, for it is well known that the milk of one cow may be in such a state that when mixed with good milk it will soon become putrescent, and this may have been going on without suspicion. I am not now called upon, and it would occupy too much time to suggest all the precautions necessary to prevent future epidemics; but this I may say, that having proved, as I think, that milk and water may be polluted by sewer gases, it is obvious that the real remedy lies in strict attention to the removal of foul odours from our drains and sewers, keeping our dairies and larders clean and sweet, and so annihilating all dangerous nuisances of that character in and about our dwellings."

And so it would appear that a theory which Dr. Hardwicke at the beginning of his paper repudiates as "extremely doubtful," is at the end held to have a probability approaching to certainty; and that while the authorities whose hasty conclusions he condemns look upon the contamination of milk with typhoid-poison as an accidental and comparatively rare occurrence, he looks upon it as an ever-present danger not peculiar to dairies, but common to all houses the drains of which are

out of order. If it be assumed that each of the infected houses in the Marylebone outbreak poisoned the milk supplied to it, Dr. Hardwicke's scientific doubts as to the medium through which the typhoid-poison was disseminated in that case will obviously be fully satisfied!

## PROSPECTS OF SANITARY LEGISLATION.

THE *Times* has taken a remarkable position with respect to projected sanitary legislation during the coming Session of Parliament. The occasion of the expression of opinion to which we are about to refer was a letter of Mr. Francis Sharp Powell, M.P., to that journal, respecting the Public Health Bill promoted by Sir Charles Adderley and himself and others last year, and which he states will be brought forward again, in substance, this year. This bill commended itself to sanitary workers from its dealing with certain much-felt defects in the law of nuisance and other parts of the sanitary laws, defects several of which it was believed would go far to neutralise the good to be obtained from the new sanitary organisation in rural districts. Among the more important provisions of the bill the following may be mentioned. It was proposed that the provisions of the Nuisances Removal Act, 1863, which empowers a Medical Officer of Health or Inspector of Nuisances to inspect certain articles of food, and seize them if found unwholesome, should be extended to milk and tea. It improved the procedure in case of the sale of unsound food, so as to make the seller responsible. Especially it proposed to add to the definition of the word "nuisance" the following:—  
(a) Any inhabited house or building without an adequate supply of wholesome water, or access to an adequate supply thereof within a reasonable distance; (b) Any inhabited house or building, or any inhabited part thereof, admitting rain or other water so as to be injurious to health; (c) Any well or pump, whether public or private, used or likely to be used by human beings for drinking purposes, or for the preparation of

human food, the supply of water from which is from any cause unwholesome and injurious to health; (d) Any pool, ditch, gutter, watercourse, privy, urinal, cesspool, drain, or ashpit, in such a condition from any cause as to be a nuisance or injurious to health, and any drain inlet requiring to be trapped and not properly trapped; (e) Any animal kept in such a situation as to be a nuisance or injurious to health.

Further, the bill provided for an extension of the right of complaint under the Nuisances Removal Acts, and for an amendment of the law of nuisance as to noxious trades and business, so that their injuriousness may be dealt with irrespective of effluvia caused by them. It provided also for an amendment in respect to evidence and proceedings in cases of nuisance, so that the effect of one nuisance may not be held to be unproved because it is not altogether separable from the effects of other neighbouring nuisances. The bill dealt also with certain hospital difficulties, providing for the usage by a sanitary authority of any hospital outside its district, if the hospital be convenient to the district; and further, enabling poor-law guardians, if they saw fit, to receive patients suffering from contagious or infectious disease into their hospitals on payment. Other important provisions related to the extension of the sections of the Sanitary Act, 1866, as to houses let in lodgings, without reference to the restriction of population; and to the giving to rural sanitary authorities the power of making bye-laws, and the enlargement of those bye-laws so as to include the following matters:—Dryness of site in the case of new buildings; structure of walls, foundations, and ground-floors of new buildings, for the prevention of damp; the use of earth closets; proper facilities for emptying and cleansing water closets, earth closets, privies, ashpits, and cesspools, &c. Further provisions had regard to the compulsory ventilation of sewers, to subjecting the sanitary authority to penalty in case of neglect of certain scavenging processes, and to the systematic analyses, by a sanitary authority, of the drinking-waters of its districts.

It is not necessary to insist upon the essential importance and great need of these different suggested provisions, although opinion may not be uniform as to the best method of dealing with some of the evils to which they relate. The *Times*, however,

while fully admitting both the importance and the need, thinks that it would be well to postpone legislation on the subject for a while, until it is ascertained how the Public Health Act 1872 is working. The *Times*, in fact, would preface further sanitary legislation by an inquiry into the results hitherto obtained from the measures adopted by Mr. Stansfeld in the initiation of the Public Health Act 1872. It holds that until the organisation carried out under this Act is completed, and the new sanitary authorities and their officers have become familiarised with their duties, additional legislation, however important, would as a rule simply give rise to additional complication and confusion in the working of the Sanitary Acts. The new organisations, so far as is known, do not inspire much confidence, and it would be well that the source of their incongruousness should be ascertained and rectified before augmenting the duties attaching to them. It would be difficult to underrate the importance of this argument, and the evidence it affords of the growing distrust and dissatisfaction with Mr. Stansfeld's public-health policy. We hope that it will bear fruit during the coming Session of Parliament.



# THE PRACTITIONER.

MARCH, 1874.

## Original Communications.

### ON SYMPATHETIC OPHTHALMIA, WITH AN ANALYSIS OF FOURTEEN CASES TREATED BY ENUCLEATION.

BY MR. W. SPENCER WATSON, F.R.C.S.

*Surgeon to the Royal South London Ophthalmic Hospital.*

IT has now become an accepted rule, perhaps almost an axiom in ophthalmology, that whenever an injured or diseased eye appears to be causing sympathetic ophthalmia it should be forthwith removed. Some surgeons even go so far as to advise removal of an eye under some circumstances *before* sympathetic irritation has shown itself, but where it is almost certain that the disease will sooner or later be induced. For instance, an injured eye in which a foreign body is known to be lodged is a source of constant danger to its fellow; and it is urged by some surgeons that, in all cases in which it is impossible or inexpedient to attempt the removal of the foreign body, the eyeball itself should be removed. Again, in cases of total destruction of sight after an attack of glaucoma or purulent ophthalmia, if the eye remains tender or painful, or both, it is probably better to extirpate the diseased and useless organ *before* sympathetic irritation of its fellow has commenced. And the reason of this is sufficiently obvious. Prevention is better than cure. Sympathetic ophthalmia once started is a most intractable malady

and cannot always be arrested by the enucleation of the fellow eye, even when this remedy is followed by most energetic local and constitutional treatment.

Hence it may be laid down as a *rule* of surgery to enucleate an injured or diseased eyeball whenever sympathetic ophthalmia is present or may be anticipated. But the discretion of the surgeon will decide in each particular case how far he is justified in the anticipation of a future sympathetic ophthalmia. Several of the cases in the Table (especially Cases 2, 3, 6, 7, and 10) illustrate the danger of delay, at the same time that they also show the possibility of a long period of immunity from the anticipated danger. "If," a patient might argue, "my eye will last me twenty-five or even twenty-eight years (as in Cases 2, 3, and 10), I may as well avoid the severe operation of enucleation as long as possible, and submit to it at last, when I find the sight of my sound eye failing." There are, however, several fallacies in such a train of reasoning.

(1.) The operation of enucleation<sup>1</sup> is *not* a severe one, under the anesthetics now employed, and is never followed by any serious results.

(2.) The longer the operation is delayed, the greater will be the danger of uncontrollable cyclitis being set up. At any time the general health might become temporarily impaired, and the chances of cyclitis coming on would be much increased. It is noteworthy that in the three cases in which the disease had been of longest continuance, bony deposits were formed. It is probable that these deposits in the course of growth would be additional sources of irritation, acting like foreign bodies on the ciliary nerves. Hence an operation undertaken before time had been allowed for the deposition of bone in the choroid would be more likely to prevent mischief than if they had long been deposited, and had slowly but persistently irritated the eye and its fellow.

(3.) It is observable that in two of the cases operated on twenty-five years after the injury, the sight was only partially restored, in both the *field of vision* being much contracted. In Case 7 the operation barely saved the sight, and only after a

<sup>1</sup> Bonnet's operation was employed in all the cases in the Table, and is the one universally adopted by surgeons in the present day.

long course of mercury and iodide of potassium; and in Case 14 the recovery of sight was very slow, and only very imperfect. In both these cases *years* had elapsed since the primary mischief, but it came at last, and was then very severe and intractable. Hence an early operation is always preferable to delay, and as no harm can result from a too early interference, it is a *rule to operate even when in doubt* as to its absolute necessity.

It sometimes happens that the injured or diseased eye retains some amount of, or even *good*, vision, and here of course delay is most tempting. I myself would never advise enucleation for *prevention* of sympathetic cyclitis in a case in which there was really useful vision in the injured or diseased eye. If, however, the sight were so far lost as to be useless, the case would be the same as that of a blind eye.

When, however, sympathetic irritation, as evidenced by intolerance of light, fatigue in reading, lacrymation, impaired vision, aching, &c., had once commenced, it would be justifiable, I think, to enucleate an eye with even a considerable amount of useful vision. Nevertheless it would under such circumstances be desirable first to attempt treatment for the improvement of the general health, at the same time keeping both eyes shaded strictly from light, and giving them absolute rest. If, after a fortnight or three or four weeks of such treatment, no improvement occurred, the operation of enucleation should be resorted to, though possibly in such a case division of the ciliary nerves by Meyer's method might first be tried, leaving the more formidable proceeding as a *dernier ressort*. I have only tried Meyer's operation in one case (Case 1), and it certainly promised well at first, though ultimately enucleation was required from a recurrence of the symptoms. It can only be applicable to a very small number of cases, those, viz., in which the irritating influences are confined to a certain quadrant or other small arc of the ciliary region. In the majority of cases the congestion and tenderness of the ciliary region extends around the entire circle, and it would be obviously impossible to divide all the ciliary nerves without at the same time performing abscission, an operation entirely unsuited for the cases under present consideration.

The state of general health often seems to have a decided influence in determining the outburst of sympathetic cyclitis. In Case 2 the patient was intemperate and out of condition; in Case 11 the health was broken by syphilis; and in Case 14 the health was very feeble, the man's complexion being pallid and flabby, his temper extremely peevish, and his manner anxious and nervous.

In all these three cases (and I think in others in which the circumstance was not noted) the injury had been almost forgotten until the outburst of cyclitis, determined by the lower state of general nutrition at the time.

The use of mercury after enucleation was well illustrated by Case 7. No immediate benefit resulted from the operation, partly no doubt in consequence of the obstruction to vision by the synechiæ and effused products in the deeper structures, and partly from the persistence of the morbid action. The removal of these products and the arrest of the cyclitis could only be due to the mercury and iodide of potassium, though the enucleation was quite necessary as a preparation for this treatment.

In Case 14 opiates were of great service in allaying the irritability and restlessness due to long-continued pain and anxiety. I have often found some form of opium extremely useful in similar cases, and if from any cause it is necessary to delay operating, when it is clear that the operation will be ultimately required, the use of opiates at night gives great temporary relief to the patient, and should be persisted in for some time after the operation, in conjunction with other treatment directed against any constitutional dyscrasia that may be present. If, however, the restlessness is found to depend, not upon pain, but upon nervous irritation and exhaustion, chloral hydrate answers extremely well, and does not give rise to the disturbance of the digestive organs and bowels that often follows the use of opium.

Whenever there has been decided cyclitis in the uninjured eye, it is better to defer the fitting of an artificial eye till this has subsided. Otherwise, the sooner an artificial eye is applied the better will be the cosmetic effect. The longer it is delayed the more the eyelids shrink and fall in upon the orbit; and when this shrinking has gone on to any extent, it is impossible

for the patient to wear a full-sized eye. The consequence is, that the small size of the artificial eye as compared with the sound eye at once attracts attention and is very unsightly. But this is a matter of secondary consideration as compared with the preservation of the sight of the uninjured eye, and the cosmetic effect must be sacrificed if it seems at all possible that wearing an artificial eye is irritating or likely to keep up sympathetic cyclitis.

In considering the advisability of removing an eye in which a foreign body is known to be present, it is well to remember that foreign bodies have been removed without resorting to enucleation, and without any ill results following to the uninjured eye. Thus Mr. Dixon was fortunate enough to remove a chip of metal from the vitreous, having previously discovered it by ophthalmoscopic examination.<sup>1</sup> And several instances are on record of the successful removal of foreign bodies lying on the iris and in the substance of the lens. The following case occurred in my own practice. A man received part of the charge of a gun loaded with small shot in his eye. Sight was at once destroyed. He was sent to me by Mr. Ireland, who at first attended him, and from the appearance of the eyeball and the position of the wound in the sclerotic, it was evident that a shot had lodged itself in the vitreous or close to the vitreous in the ciliary region. The shot could not be seen by the ophthalmoscope, but effused blood was seen lying at the lower part of the vitreous space behind the iris. Wishing, if possible, to avoid enucleation, but at the same time preparing the patient for the possible necessity of enucleating, I at first made a large iridectomy downwards, in the expectation that the large pupil thus obtained would give a more perfect view of the vitreous chamber, and perhaps allow of a subsequent operation for the removal of the foreign body. While in the act of making the first incision, the shot rolled out into the oculo-palpebral fold, and thus solved the difficulty. The patient made a good recovery, the eye, of course, remaining blind and with a displaced pupil; and no sympathetic irritation had shown itself in the fellow eye. Two years after the accident, the sight of the uninjured eye was as good as ever.

<sup>1</sup> See *Ophthalmic Hospital Reports*, vol. i. p. 80.



*Fourteen Cases of Sympathetic Ophthalmia, treated by Enucleation of the Injured or Diseased Eye primarily affected,*

*Abbreviations employed in this Table:—M., male; F., female; R., right eye; L., left eye; V., acuteness of vision; F., field of vision; J. 1, J. 2, &c., the test-types of Jaeger.*

No.	SEX AND AGE.	CONDITION OF PATIENT.	STATE OF INJURED OR WASTED EYE.	PERIOD AFTER INJURY OR DISEASE.	STATE OF V. BEFORE OPERATION.	STATE OF V. AFTER OPERATION.	OBSERVATIONS.
J. II. Case I.	M. Æt. 28 yrs.	Boiler-maker, in good health before accident.	Soft, painful, and tender. Cataract and synechia. Sees movement of hand.	Struck in L. 4 months before.	Photophobia & circumorbital pain, V. much impaired.	V. restored sufficiently to enable him to resume his work.	The operation recommended by Dr. E. Meyer, of dividing the ciliary nerves of the injured eye, was first tried, but only gave temporary relief. Enucleation was subsequently employed with good results. (See report in Brit. Med. Journal, Oct. 16, 1899.)
— Tofts. Case II.	M. Æt. 36 yrs.	Brewer's drayman. Intemperate and out of condition.	Superficial chalky opacity of cornea. Lens converted into dense earthy material.	Blow on brow with a stick 25 years before.	Photophobia & pain, increasing dimness of V.	V., J. 2. F. limited.	On removal, the eyeball contained a cup of true bone lining the sclerotic. The lens was converted into a dense, porcelainous mass. (See Path. Trans., vol. xxii., p. 225.)
Rich. Wright. Case III.	M. Æt. 40 yrs.	Labourer in country.	Shrunk, and painful eyeball.	25 years.	Impaired V.	V. improved for some months, subsequent relapse, and return of dimness. Centre of F. entirely lost.	A rounded ossile deposit found in the eyeball removed. The patient, on his return to the country, used his eye incautiously and without a shade; and hence, probably, the partial want of success in treatment.
M. A. Darwin. Case IV.	F. Æt. 43 yrs.		Shrunk and drawn back into orbit. No trace of cornea.		Photophobia & asthenopia. Impaired V.	V. much improved. V., J. 2.	When seen five months after, the improvement of V. had continued up to within a fortnight, and the temporary relapse was due to conjunctivitis in the socket of the eye removed,—possibly from irritation caused by the artificial eye.
Case V.	M. Æt. 42 yrs.	Labourer in country.	Stump of size of a bean.	1 year. Lost by ophthalmia, not by injury.	Photophobia & impaired V. Pain.	V. improving two months after enucleation.	The eyeball removed contained a cup-shaped deposit of bone around the optic nerve, coated externally with choroidal pigment. (See Path. Trans. vol. xxii., p. 226.)
John Greenfield. Case VI.	M. Æt. 46 yrs.	Soldier. Had been in East Indies.	Shrunk and tender, mere trace of cornea left.	8 years. Burst by a blow of fist.	Photophobia. V., J. 16 at 4".	No photophobia. V. J. 13.	The improvement was not great immediately after the operation, but still it was very decided.
A. Champion. Case VII.	M. Æt. 36 yrs.	Porter.	Shrunk and tender.	5 years. Injured by a man's finger thrust into it.	V. not letters of large type. Acute cystitis and keratitis.	V. not improved after enucleation. After a mercurial course, V., J. 8.	The cyclitis seems to have been rekindled by a second injury to the eye not at first injured, about a year and a half before enucleation. The benefit of the mercurial course, followed by iodide of potassium, was very marked. The eyeball removed exhibited the infundibular coarcted retina with fluid between it and the choroid.

Case VIII.	M. Æt. 12 yrs.	Lost by purulent ophthalmia in early infancy. Cornea dull and opaque in centre.	12 years.	Photophobia. V. much impaired.	V. improved, but remained very impaired.	The improvement of sight lasted nearly a year. The boy was then, unfortunately, again injured in the surviving eye by a gunpowder accident, but the effects were only transient. He soon regained his sight, slight nebula only remaining.
Robert Hies. Case IX.	M. Æt. 55 yrs.	Lost by acute ophthalmitis. No injury.	7 years.	Photophobia.	Less photophobia.	The patient only under observation for three weeks after the operation. Sight was then improving.
Case X.	M. Æt. 54 yrs.	Injury by shot. Globe shrunk, painful, and tender. Cornea opaque.	28 years.	Photophobia. V. very much impaired, pupil being almost closed by synechie, mu- seu, and photophobia.	V. good enough to allow of shootings, and reads J. I.	A shot found in the eye, and bony deposits also. (See Path. Trans., vol. xx., p. 303.) The patient had not suspected the presence of the shot, as, at the time of the accident, two grains of shot were removed from the eye or eyelids. For more than twenty years, however, no sympathetic irritation had shown itself.
Case XI.	M. Æt. 22 yrs.	Irritation and tenderness of injured eye. Tenderness at upper and inner ciliary region.	Several years before injured by piece of percussion cap.	Photophobia & impaired V.	Some improvement.	The condition of the patient's health clearly influenced the course of the disease. For some years no sympathetic mischief had arisen, and then became lighted up by the attack of syphilitic iritis. The improvement subsequent to enucleation was at first only slight, but ultimately became more decided.
B. Cossingham. Case XII.	M. Æt. 34 yrs.	Globe sightless, shrunk, soft, congested, and painful.	1 month. Injured by a chip of metal while hammering an "iron chair."	V. impaired, and great photophobia and pain.	V. improved. V. J. I. Photophobia gone.	A chip of iron was found embedded in the vitreous, and surrounded by puro-lymph. The rapid and complete relief of all the untoward symptoms immediately after enucleation was very marked. (Reported in <i>Lancet</i> , May 10, 1873.)
Case XIII.	M. Æt. 40 yrs.	Globe shrunk, congested, and painful. Cornea clear. Cataract.	Months. Accute suppurative ophthalmitis. No injury.	V. J. 20, with effort, and photophobia.	V. J. 16, and improving. A fortnight after enucleation, V. J. 14, and no photophobia.	The improvement in vision was very soon observable; but was the more unexpected because the ophthalmoscope revealed great venous engorgement and congestion of the optic disc, and floating crystals of cholesterolin were seen in the vitreous of the injured eye.
J. G. Case XIV.	M. Æt. 46 yrs.	Globe tense, affected with absolute glaucoma, and sightless.	6 or 7 years ago sight lost by glaucoma 1 month.	V. much impaired. Extreme photophobia.	V. not improved for many months, but ultimately restored so as to be useful in his business.	After the enucleation of the glaucomatous eyeball, the patient was put under a long course of bromide of potassium, with atropine to the eye and fying blisters to the temples. As his general health improved the photophobia gradually subsided, and vision became restored. He was evidently suffering from some chronic derangement of the nervous system, and this complicated the case throughout. The enucleation was evidently necessary as a preliminary to general constitutional treatment.

These cases (p. 165) must be very exceptional, and cannot be taken as guides for treatment. In the last case, good fortune rather than skill was the cause of the good result. If the incision had not happened to have been exactly at the point at which the foreign body lay, there would have been probably an increase of irritation, and enucleation would have been necessary. There might have been more than one foreign body, and this again would have led to the same necessity; but still the possibility of saving an eye under favourable circumstances, such as those mentioned, should always be kept in view, and the more radical measure can be resorted to if the preliminary operation fail. But in a few rare cases it may be possible to save an eye in which a foreign body *is seen* without resorting to any operation. I have now under observation a young man in whose vitreous I can see a small chip of metal suspended in the vitreous humour. It is exceedingly minute, and can only be seen by using a magnifying glass, but it is nevertheless evident enough, and its position and metallic character distinct enough, to make me sure of its lodgment in the vitreous. The scar of the point of entrance is also evident, and the history of the accident satisfactorily accounts for its presence. At first some irritation of the eye showed itself, but under treatment by atropine and cold applications all irritation subsided, and vision is perfectly good. I have therefore decided to leave this foreign body where it is. It is suspended in the vitreous, which is elsewhere perfectly clear, and there are no signs of irritation in any of the more sensitive and vital parts of the interior of the eye. A small blood-clot near it has become smaller and more attenuated, and the retina is quite unaffected. It will therefore be quite unjustifiable to interfere in this case by operation, unless any change takes place in the position of the chip of metal, or any inflammatory exudation occurs in its neighbourhood. Its extremely small size is an important element, because its weight will not be likely to cause any change of position, and the consistence of the vitreous will be sufficient to hold it away from any important parts.

This again is an exceptional case, but others are recorded in which metallic bodies of larger size have remained embedded even in the retina for a considerable time without causing any

serious mischief. The liability to mischief occurring at any time under such circumstances should be plainly stated to the patient, and he should be examined from time to time, as irritation might suddenly come on, and sympathetic mischief would very soon follow. Hence it will not be justifiable to lose sight of such a case without warning the patient of the danger he incurs, and placing before him the possibility of his having to submit to enucleation, if symptoms require it.

## SOME REMARKS ON THE THEORY OF INHIBITORY OR REFLEX PARALYSIS.

BY C. HANDFIELD JONES, M.B. CANTAB., F.R.S.

IN the *British Medical Journal*, 1874, i. p. 40, the following passage occurs in a highly interesting lecture on diabetes by Dr. L. Brunton:—"The second way in which the hepatic vessels may be dilated is by reflex paralysis, or inhibition, as it is generally termed, of their vaso-motor nerves. Everyone knows that when a sensory nerve is irritated, the impression is transmitted to the vaso-motor centre, and arrests its usual action over the vessels of the part to which the sensory nerve is distributed. Thus, when a grain of sand falls into the eye, the irritation which it occasions to the sensory nerves of the conjunctiva is conveyed by them to the vaso-motor centre, and arrests the action of that part of it which regulates the contraction of the conjunctival vessels. In consequence of this, they become dilated and full of blood, and continue so while the irritation continues; but so soon as it is removed the vaso-motor centre again regains its wonted power, and the vessels return to their normal size. The same is the case with the liver, and its sensory nerve is the pneumogastric." Lovén's experiment on the auricular nerve of the rabbit, Rutherford's demonstration of the inhibitory action of the vagi on the gastric blood-vessels, Bernard's of that of the gustatory nerve on the vessels of the submaxillary gland, and Cyon's of the action of the depressor nerve of the heart on the intestinal arterics through the splanchnics, constitute, I suppose, the chief experimental evidence in favour of the view above stated, which



now seems to be reckoned among the accepted doctrines of physiology. As Dr. Brunton takes his illustration of the mechanism of the process from pathology, it may be concluded that he considers the same view to be applicable to morbid as well as to normal actions.

Admitting this, it may however be well to point out in what respects pathological differ from physiological inhibitory actions: (1) The immediate motors of the former are not normal and appropriate stimuli, but injurious irritants, or depressants. (2) The paralyses are not confined to vaso-motor nerves, but affect also musculo-motor, common and special sensory, and even the hemispheres themselves. (3) The districts affected are often non-coterminous with, perhaps remote from that occupied by the incident nerve and its ramifications. (4) The occurrence of pathological inhibition may depend not on an absolutely excessive or injurious irritation having been applied, but on the incident nerve, or the recipient centre, being previously in a morbid state, hyperæsthetic, or hyperexcitable. (5) The same irritation may affect (pathologically) different parts in different persons. (6) Physiological inhibition is essentially transitory; pathological continues as long as the irritation persists. (7) Physiological inhibition is requisite for the performance of some function; pathological conditionates disease, disorder of function.

Sufficient heed has not always been given to the difference which may exist between the effect of different kinds of excitants. Just as we know it is with mental influences, some of which exalt and intensify nerve-force, to a high degree, while others as powerfully depress it, so it is probably with physical. One which is appropriate in kind and degree may be a very beneficial stimulant; another which is too powerful or altogether pernicious will have the most opposite effect. Thus it certainly is with wine and electricity: both in suitable doses are recreative; both in excessive are ruinous to healthy life. A carious tooth and a warm condiment are no doubt both excitants of the same sensory nerve, but how very different are the impressions made on the nerve-centre in the two cases! All impressions on sensory nerves certainly do not paralyse the associated vaso-motor. Vasa nerve-centres appear to be

more readily paralysed than any other; which may depend partly on their being smaller, and therefore more easily deranged than larger. It may also be the case that the resulting hyperæmia is more easily appreciated than diminutions in the force of muscular masses, or in the tactile faculty of the integument. Some morbid phenomena of inhibition produced by disease are almost as precise and significant as the results of experimentation. It may be well to enumerate a few for the sake of those to whom the idea is not familiar:—

(1.) Crimson flushing of head, face, and neck from gastric irritation, itself perhaps depending on gastric hyperæsthesia, unilateral flushing and heat, sweating of face from loaded bowels, face hyperæmic and covered with sweat when prepuce was irritated in a case of preputial neuroma. (*Vide Verneuil, Year-book: Syd. Soc.* 1862, p. 239.)

(2.) Salivary flux from uterine irritation in pregnancy, or from neuralgia of fifth, or from irritation of filaments of the vagi distributed to the œsophagus. (*Vide Dr. Fussell's case in Lancet*, 1873, ii. p. 625).

(3.) Paralysis of heart by gastric, intestinal, cutaneous, or urethral irritation, as in gastralgia, peritonitis, burns, and operations on the urethra.

(4.) Paralysis of vasal nerve-centres from stricture-splitting, with coma; temperature  $107^{\circ}6$ . (*Vide Medical Times and Gazette*, 1873, ii. p. 121).

(5.) Sensory paralysis, as in Roche's case (*vide Brown-Séquard's Phys. of Central Nervous System*, p. 131,) and Sir Thomas Watson's case (*Lectures*, last edition, vol. i. p. 538).

(6.) Motor paralysis, *vide* instances of reflex paraplegia given by Brown-Séquard; a case, cited from Mr. Morgan, of stone in bladder, *vide F. N. D.* p. 140; a case of my own, at p. 115, of left hemiplegia cured by an emetic; case of same ceasing after delivery, *vide* p. 119; cases of palsy from exposure to cold; cases of paralysis of muscles of eye in neuralgia of fifth.

(7.) Cases of amaurosis from gastric or dental irritation.

Those who hold with me that pain is a mode of sensory paralysis will see in the common instances of reflex, or remote pain, a phenomenon closely analogous to, if not identical with, inhibitory paralysis.

The first observer who entertained the idea of reflex paralysis of blood-vessels was, I believe, Henle, who proposed a theory to this effect—that the nerves of the vessels are in antagonism with the nerves of animal life, especially with the centripetal, so that in proportion as the latter are excited, excitement ceases in the former.<sup>1</sup> This view was adopted by Mr. Simon in his admirable lectures on pathology, where he says (p. 80) that “reflex-relaxation” (as he calls it) appears to him “the only plausible explanation of the condition of the larger blood-vessels in active hyperæmia, whether inflammatory or hypertrophic.” Subsequently the views developed by Weber, Pflüger, Rosenthal, and Nasse, as to the existence of certain systems of inhibitory nerves, contributed to familiarise the minds of inquirers with the idea that one nerve might diminish the action of another. Lister’s researches confirmed and corrected these views, showing that the same afferent nerve might enhance or inhibit, exalt or depress, the functions of the nervous centre on which it acts, according as the stimulus applied to it was mild or potent. His paper was published in 1858. Bernard’s essay on the influence of paralysing reflex agencies is dated September 1864; Lovén’s paper appeared in 1867; Rutherford’s in 1870. My views were first published in February 1859 in the *British Medical Journal*, and subsequently developed more completely in my Lumleian Lectures, *Medical Times and Gazette*, 1865, and in *F. N. D.* 1864 and 1870. They have been well appreciated by Anstie, but are scarcely mentioned by anyone else. Brown-Séquard, in his lectures published in the *Lancet*, 1860 and 1861, laid great stress on the production of paralysis, either in the cord or in the brain, by remote irritation; ascribing it, however, rather to anæmiating spasm of the vessels of the paralysed centre than to a direct action of the afferent nerve on the nerve-cells. Though I differ from him on this point, I think he has contributed very highly, perhaps more than anyone else, to establish the doctrine of reflex or inhibitory paralysis. His extension of this view to those cases where head symptoms—paralysis, &c.—cannot be explained by any discoverable destruction of the organ of the will or of the conductors between it and the muscles, seems to me quite correct, and a step of no mean importance. Henle’s

<sup>1</sup> *Traité d’Anat.* vol. ii. p. 58.

original view, important and suggestive though it be, and supported by Lovén's experiment, does not seem to be substantiated. All excitements of afferent nerves do not dilate blood-vessels. Cold operating reflexly certainly does not. Nor does heat invariably, for Trousseau found hot water a more efficient styptic in epistaxis. Neuralgic perturbation does not induce hyperemia in the majority of cases. Local irritants generally confine the resultant hyperemia to the area on which they act, and produce no general flushing of the adjacent surface. Their action seems to be rather on the tissue than on the vessels. When the skin on being scratched with the finger-nail presents the "tache méningitique," it is difficult to think that the lines of redness can be produced by any paresis of vaso-motor nerves. If such occurred in a reflex manner, it surely could not be so limited in extent, but would appear as a more or less widespread flush, as in instances cited in my Lumleian Lectures. In five instances where I faradised the peroneal nerve with moist rheophores just below the head of the fibula for five minutes, no redness of the skin of the parts below was produced, and only in two or three some filling of one or two superficial veins. Galvanisation is known to be much more effective than faradisation in causing augmented blood-flow in the district traversed by the current.

On the whole it appears, I think, that my original view is correct: that it is for the most part *morbid* excitation—*irritation* as opposed to *stimulation*—which produces reflex or inhibitory paralysis in any part. Some few instances there are of physiological inhibition, but these seem only to render it more probable that a similar effect can be produced pathologically. Lovén's experiment, on which much stress is laid, seems to me rather an instance of pathological than of physiological inhibition. If we think what a difference there must be between our rude experimental excitation of a nerve and the normal, we must admit that the conclusion arrived at by Mr. Lister, respecting the different effects of gentle and strong stimulation, is highly rational and probable.

## ON THE THEORY OF COUNTER-IRRITATION.

BY JAMES ROSS, M.D., WATERFOOT, NEAR MANCHESTER.

(*Read before the Manchester Medical Society, Feb. 4, 1874.*)

COUNTER-IRRITATION may be defined as the application of an irritant to one part of the body in order to influence morbid action in its vicinity. But when an irritant is applied at a distance from the morbid part, such as a mustard foot-bath for head affections, it does not come within the scope of this definition. Near and distant irritation do not probably act upon the same principle, and they require different practical rules for their application; it will therefore be practically convenient to separate them in our classification. *Revulsive* is a very bad term if it be held to involve a theory of the action of distant irritation in the cure of disease; but the term has been long in use, and with care in explaining that its employment did not imply any theory, it might be usefully retained. But however this question may be settled, I only mean to discuss at present the action of irritants when applied in the vicinity of the disease.

I shall now mention briefly the local effects which follow the application of a counter-irritant. I say the effects, because the prejudicial effects must be noticed as well as the beneficial; since we have not only to frame positive rules for the application of counter-irritants, but also negative rules for abstaining under certain circumstances from their employment. And indeed one of the first rules with regard to counter-irritants laid down in practical works is a negative one of this kind. The rule is—do not apply a counter-irritant in the early stage



of inflammation. The rule would be meaningless had not experience taught us that there is very considerable danger lest the counter-irritant should aggravate the disease. It may be concluded, therefore, that a counter-irritant may aggravate the disease if applied during the first stage of inflammation, although this is by no means always the case.

There is a pretty general agreement in the opinion that counter-irritants tend to deflect the part towards health when applied during the second stage of inflammation; and more especially is this the case when the disease becomes chronic. Even here, however, the effect is not always beneficial. If a part in the second stage of inflammation is on the verge of breaking down into pus, the application of a counter-irritant may accelerate the process; and whether this event is or is not desirable, the deflection produced cannot be said to tend directly towards health. But counter-irritation is also used with marked benefit in cases which are not of inflammatory origin. Such cases as flatulent colic and partial paralysis of the bladder may be mentioned as examples. In these diseases the main symptoms arise from debility of the muscular coats of the respective organs, and it is important to remember that counter-irritants tend to restore health in such diseases.

There are two other important points which must be noticed in connection with the practice of counter-irritation. Quantitative differences may be observed in the effects according, first, to the proximity of the irritant to the seat of the disease; and, secondly, to the degree of irritation produced. If the irritant is placed too far from the seat of the disease, synovitis of the knee-joint for instance, it will have little or no effect; if at a moderate distance it may have a curative effect; and if too near it may aggravate the disease. Qualitative differences obscure to some extent the comparison, nevertheless it is readily seen that between no effect, a curative effect, and an aggravation of the disease there must be quantitative differences. The quantitative differences which arise from the degree of irritation are also considerably obscured by qualitative differences. But the fact of our dividing counter-irritants into rubefacients, vesicants, pustular counter-irritants, issues, &c., along with the fact that some are called milder, and others severer, counter-irri-

tants, and that the former are used in acute and the latter in chronic cases, is alone sufficient to show that there are quantitative differences in the effects produced by different degrees of irritation. These are the main inductions which a survey of the effects of counter-irritants affords. How shall we interpret them?

Some would place the old maxim "*Ubi irritatio, ibi fluxus*," at the basis of any explanation of the action of counter-irritants. But although this maxim is perfectly true in so far as it is a statement of fact, it explains nothing unless it can be shown that the special flow of blood to the irritated part is accompanied by a special withdrawal of blood from the surrounding tissues. It cannot be shown that this is the case even when the part irritated and the part diseased have a direct vascular connection, and much less is it likely to happen when no such connection exists. But even if blood could be withdrawn from a diseased part, the effect produced would be very unlike that of counter-irritation. Imagine a practitioner giving the following instructions to his pupil:—Withdraw blood from organs during the second stage of inflammation, especially in chronic cases, and also from organs suffering from local debility, and this will tend to cure them. You exclaim, "How exceedingly false!" But let our imaginary teacher substitute counter-irritation for local blood-letting, and you are almost ready to exclaim, "How exceedingly true!" Practically the distinction between counter-irritation and local blood-letting is acknowledged, yet theoretically both are explained upon the same principle. Our precepts for the guidance of practice in local blood-letting are almost directly opposed to those for counter-irritation, but what are considered contraries in practice are regarded as identicals in theory. When I say that counter-irritation and local blood-letting are regarded as contraries in practice, I am alluding to practice as it exists at the bedside, and as I can gather it from practical precepts, and not as I find it reflected in the classification of remedies. The practical precept warns us not to apply a counter-irritant near an actively inflamed organ, lest the disease be aggravated; but in classification counter-irritants are called antiphlogistics. The precept is the reflex of practice, the classification the reflex of theory.

Antiphlogistic, forsooth, when we are warned in the same breath not to employ the remedy in an early stage of inflammation. A counter-irritant might more properly be called a *phlogistic* than an *antiphlogistic* remedy.

The idea that counter-irritants are antiphlogistics has, however, got such a deep hold of the medical mind that those who see clearly that the flow of blood which takes place to a newly-irritated part will not directly diminish the flow of blood to the surrounding textures, endeavour to supply a machinery which they suppose to produce the same result indirectly. Such do not doubt that an artificial irritation does diminish the flow of blood to a diseased organ in its vicinity, and, since this cannot be explained upon hydraulic principles, it is supposed that it must be explicable by the indirect action of the nerves. Irritation of the sympathetic at a certain point induces a contraction of the surrounding minute arteries, and this of course lessens the supply of blood to the surrounding textures. It is difficult to understand how the minute arteries of the lungs should be specially contracted by the application of a counter-irritant to the surface of the chest; but admitting that such an action takes place, it would not account for the effects of counter-irritants. Contraction of minute arteries might be of some use in the first stage of inflammation; but a counter-irritant may aggravate the disease in this stage. On the other hand, what would be the use of contracting the blood-vessels in the second stage of inflammation, when a large number of the capillaries are already occluded? In this stage we want more, and not less, blood to the part. And lastly, in cases of debility, where counter-irritants are found so useful, it would be madness to endeavour to contract the blood-vessels even if it were possible.

The theory I advocate is, that a counter-irritant always tends to stimulate the neighbouring textures to increased activity, and that this stimulating action spreads along the parenchyma not merely when the tissues are continuous, but also when they are simply in contact. Let us attend to the successive changes which take place when a part of the surface is irritated, as for instance by the application of tartar-emetie ointment. In the centre there is a small cavity filled with pus. Surrounding this cavity there is a zone of tissue thickened by new deposit. Still

further from the centre there is another red zone exhibiting vascular engorgement, and gradually fading into the colour of the surrounding skin. But it is not at all probable that the influence of the inflammatory centre ceases when we fail to recognise any engorgement of the blood-vessels with the naked eye. The overgrowth of hair which is found round old-standing ulcers shows that a certain influence is propagated much further than the visible signs of inflammatory action. It may, therefore be inferred that beyond the red zone by which an irritation merges into the healthy colour of skin, there is another zone the tissues of which are stimulated to increased action. But not only does irritation spread along the surface, but it also spreads inwards. That this is the case may be concluded from the fact that internal irritation has frequently been noticed to spread outwards to the surface. Witness the red and tender scrotum after injection of tincture of iodine for the radical cure of hydrocele. I have seen a red and tender spot on the skin of the upper lip corresponding to a long-continued ulcer on the internal surface. Again, in a post-mortem examination, a large patch of red and injected peritoneum has been found corresponding to the area of a blistered skin. There is also abundant evidence to show that irritation spreads from one tissue to another with which it is only in contact. In a post-mortem examination it is rare to find indications of previous inflammation in the pleura costalis without indications of a corresponding action in the pleura pulmonalis. Occasionally, a patch of adhesion has been met with between the pleuræ opposite a single spot of tubercle in the lung. This shows that the irritation has spread from the tubercular centre to the pulmonary, and from the pulmonary to the costal pleura. In ulcerative stomatitis, the cheek and tongue opposite to the ulcerated gum generally become inflamed and ulcerated, and this has a like implication with the other instances adduced.

Let it now be granted that the transmission of irritation from one part to another is mainly an action of the parenchyma, that irritation will pass from one tissue to another with which it is contiguous, and that surrounding the irritated part there is an area stimulated to increased nutritive activity, several questions arise. Does the area of stimulated tissue extend far enough



and in a sufficiently short time to account for the action of counter-irritation? If so, is the intensity sufficient to produce the effects? and, lastly, is the quality of the action assumed of such a nature as to account for all the local effects counter-irritants are known to produce? There are no means by which we can measure how far the excitement of tissue extends round an irritated part. Dr. Beale has, however, made a very beautiful observation which shows how very sensitive the organism as a whole is to changes going on in a single part. He says that if one has only a slight catarrh, the bioplasm (protoplasm of others) over the entire body is increased. If such is the case there can at least be no difficulty in conceiving that the special excitement which surrounds an irritated part will extend sufficiently far, and in a sufficiently short time, to account for the action of counter-irritants.

But even if it be granted that this stimulated area extends far enough to account for the action of counter-irritants, it may be objected that the action is so very slight that it could not have any appreciable effect. If a blister is placed upon the chest of a healthy person, it produces no recognisable effect upon the lung beneath, and it may therefore be supposed that it cannot have a curative effect upon a diseased lung. This is an objection which may be urged against any theory of counter-irritation; but a solution of the difficulty will be another argument in favour of the one advanced here. In a state of health the lung is in equilibrium with every part of the body, and it can therefore resist unusual incident forces of considerable intensity without a deviation from health being produced. When, however, a deviation from health is once produced in the lung, an *unstable* equilibrium is produced between it and the rest of the organism, and, as is usual in such a case, slight incident forces will produce large effects. In health, therefore, the effect produced by an irritation may be inappreciable, while in disease the effect may be very marked either for good or for evil.

The last question to determine is, whether the action assumed to spread along the parenchyma is of such a nature as to account for all the local effects of counter-irritation? The influence which I have supposed to proceed from the artificial irritation



is a stimulation of the surrounding tissues. Now suppose that a part in the vicinity of the artificial irritation is in the first stage of inflammation; it is evident that the disease may be aggravated by the slight additional stimulation. But this does not always happen. It is possible that under certain circumstances the little additional excitement may cause what is termed effusion, and the second stage of the disease to be developed. In the first stage of pleurisy, for instance, a blister on the chest soon relieves the pain; but relief is obtained not because the disease is "allayed" or "controlled," but because the pleuræ become separated by a layer of fluid; in short, the counter-irritant has not checked the diseased lung, but merely assisted it through its natural progress. In the second stage of inflammation a great many of the capillaries are occluded, and the proper parenchyma of the part must necessarily suffer from insufficiency of nourishment; hence a gentle stimulus is necessary in order to deflect the nutrition towards health. If, however, the proper structure of the part is on the eve of breaking down into pus, the additional stimulus of a counter-irritant may accelerate the event; and this may or may not be desirable in the treatment of a case according to circumstances. It is very evident that a stimulant action is very likely to do good in cases of chronic inflammation and of local debility; while it is scarcely necessary to add that this theory will account for the quantitative differences which occur in the effects of counter-irritants according to their distance from the primary disease, and according to the degree of irritation produced. This theory, therefore, gives complete unity to our conceptions of the mode of action of counter-irritants, and the practical precepts which guide us at the bedside may be readily deduced from it. This is the highest verification which is possible to be given to the theory.

## ON "REST" IN THE TREATMENT OF CHEST AFFECTIONS.

BY FREDERICK T. ROBERTS, M.D., B.SC., M.R.C.P.

*Assistant Physician to University College Hospital, and to the Brompton Consumption Hospital.*

THE attention of the profession has been recently drawn to the question of the advantages to be derived from the employment of rest in the treatment of pulmonary phthisis, by some papers published in the medical journals, particularly one from the pen of Dr. Berkart in the *Lancet* of October 18th, 1873. I propose in the following remarks to take a more comprehensive view of the subject, first offering a few general observations upon it, and then considering the matter with reference to particular chest affections, phthisis among the number. My reasons for taking part in the discussion are, that for several years I have studied the question with some care, and have made rest a prominent element in the treatment of pulmonary affections; that the majority of the profession certainly do not at present recognise in a practical manner the benefits which it is capable of affording, and therefore cannot be too often reminded of them; and that certain of the statements made in some of the communications already alluded to seem to me to call for further comment and consideration.

Beginning, then, with the general principle of rest as applied to the treatment of diseases of the respiratory organs, and of the other structures engaged in the performance of the respiratory functions, it will be well to point out briefly, first, the objects to be aimed at in attempting to carry out this principle;

and secondly, the methods by which the desired rest may be secured. One of the most important and most obvious ends to be sought after is to maintain structures, which are either actually diseased or in danger of becoming so, in as quiescent a state as possible; in short, to try to procure mechanical rest, just as is ordinarily done in the case of a diseased joint. This applies to the structures forming the chest-walls, to the pleuræ, and to the lungs themselves. A further object, also of importance, in restraining the respiratory functions, is to prevent, so far as is practicable, the contact with diseased pulmonary tissue of air, which, either from its own physical conditions or from being contaminated with abnormal and injurious gaseous or floating solid elements, may prove a source of irritation, and thus tend to increase the mischief. Again, bearing in mind the influence which the movements of the lungs during breathing exercise over the circulation of the blood through these organs, it is not improbable that by limiting these movements, either generally or locally, and thus rendering the circulation less active, some effect may be produced in the way of checking certain morbid processes, which an over-active circulation tends to promote. Lastly, some writers have attached much importance in certain cases to the diminution in the oxygenation of the blood and tissues, which necessarily follows any restriction in the performance of the respiratory functions, considering that undue oxidation is injurious, and that it hastens the progress of disease. Upon this question I shall have more to say hereafter.

Coming now to the methods which are open to us for procuring rest in connection with the respiratory organs, it may be remarked generally that the immediate objects which these have in view are either the limitation of the respiratory functions as a whole; the checking of certain acts in which the lungs are called into violent play; or the interference with the movements of limited portions of these organs or of the chest-walls. At present it will suffice merely to indicate what these methods are, as they will call for further consideration when treating of individual diseases. 1. The activity of the respiratory functions will of course depend upon the degree in which the body is exercised, and the amount of muscular effort which is put forth. Hence, by diminishing this, and especially avoiding all kinds of

violent exertion, breathing may be kept within proper limits, and it may be desirable to keep patients completely at rest, so as to reduce to the minimum the call made upon the lungs. 2. By voluntary effort on the part of the patient, the respirations may be considerably reduced in number, and limited in depth; and further, it is possible to breathe in such a manner as to use only certain portions of the lungs, leaving other portions in a state of repose. In some cases it is advantageous to draw the attention of patients to this matter, who can thus assist in the treatment when rest is required. 3. Unquestionably much harm often results, both in the way of originating morbid conditions and aiding their progress, from certain actions which call the respiratory organs into more or less violent play, and exercise a strain upon them, especially coughing, excessive use of the voice (as in public speaking, street-crying, singing, &c.), and blowing wind-instruments. Therefore it frequently becomes a matter of much importance in treatment to use every means for checking cough, and to forbid imperatively any indulgence in the violent exercises above alluded to. Indeed it is sometimes advantageous to prevent a patient from using the voice at all, quite independently of any implication of the larynx. 4. In order to avoid irritation of the lung-tissue by unhealthy air, of course the obvious duty of the practitioner is to take every precaution against the inhalation of such an atmosphere. It is clear, for instance, that if a person is, in connection with his occupation, habitually inhaling air loaded with irritating solid particles, he should at once be advised to give up such occupation, though unfortunately this advice very often cannot be followed out. In all cases of pulmonary disease, it is incumbent upon the physician to inquire into the conditions of the air which the patient habitually breathes, and to endeavour to make this as satisfactory as circumstances will permit. 5. It has been found that inhalation of compressed air has an influence in diminishing the number of respirations, both during the inhalation and subsequently; and Dr. Berkart expresses his opinion, "that the value of compressed air in the treatment of phthisis principally consists in the rest which the lungs derive from it." Finally, by the application of different kinds of apparatus over the chest-walls externally, such as plasters, bandages, &c., we possess the

power of diminishing or even completely preventing the movements of respiration, either generally, on one side, or over limited regions. This mode of procuring mechanical rest has received remarkably little attention, and though of the utmost value in many cases, has been practically overlooked by the great bulk of the profession.

I now pass on to the consideration of special diseases, in order to determine to what extent the principle of rest can be applied in the treatment of each, and how it can be realised under different circumstances.

PLEURISY.—To anyone who is acquainted with the pathological conditions resulting from this disease, it must be evident that to restrain the movements of the affected structure, to prevent the friction of the inflamed surfaces against each other, and keep the parts as much at rest as possible, ought to be the *first principle* to be followed in its treatment. The state of things is very similar to that observed in connection with an inflamed synovial membrane in a joint, and in this condition the first thing the surgeon attends to is to maintain the joint perfectly motionless. I have never seen this principle definitely advocated for the treatment of pleurisy in the published writings on this affection, and, with the exception of two or three suggestions on the subject, it does not seem to have received any particular notice. In the course of my hospital practice during the last ten years, a large number of cases of this complaint have, as a matter of course, come under my care. In the year 1864 I, quite independently, arrived at the conclusion, from certain observations, that the primary object to be arrived at in treating pleurisy should be to endeavour to procure rest; and since that time experience has fully convinced me that I was not mistaken in my opinion. I have on previous occasions brought the subject before the profession, among others in a paper on "Rest and Position in the Treatment of Medical Diseases," published in vol. ii. of the "Liverpool Medical and Surgical Reports," 1868. In my "Handbook of Medicine," lately published, the principle has been definitely laid down and advocated, and an indication given of the mode which I usually adopt in order to procure the needed rest. Now I



propose to present a more detailed account of my experience of this treatment, and of the manner in which it is carried out.

With regard to the methods employed for procuring rest in the treatment of pleurisy, that which is of special importance is the use of certain appliances fixed round the affected side more or less extensively, so as to limit or prevent its movements. As subordinate aids, it is advantageous in severe cases to keep the patient quiet; to give instructions to restrain the breathing as much as possible (though this is generally done instinctively, on account of the pain the act induces); and to forbid all conversation. In a pathological point of view the results which might be fairly anticipated from this mode of treatment are, that the inflammation would be limited and subdued; that the effusion of lymph and fluid would be checked; and that whatever morbid exudations had been poured out would be more readily absorbed, followed by organisation of the remaining lymph, with the formation of adhesions.

In actual practice cases of pleurisy present very considerable differences when they come under observation, and it will be necessary to point out to what extent the treatment by rest is applicable to the different classes. In one group of cases, of very common occurrence in hospital practice, the inflammation is localised to a small patch, and appears to have but little tendency to spread. There is generally a good deal of pain, especially on breathing or coughing, but there are no constitutional symptoms. Physical examination reveals limited friction-sound. In these instances the firm application of three or four strips of plaster round the side, in the manner to be presently described, almost invariably gives complete relief, and even allows the patient to continue his occupation. Nothing more is needed, and in a week or two the plasters may be removed. In another class of cases, a patient comes under notice who is evidently in the early stage of a severe and extensive attack of pleurisy, judging by the local and general symptoms and physical signs. It is an unfortunate fact, however, that in many instances the symptoms are not very prominent at first; and it is by no means uncommon to find that abundant effusion has taken place before the patient is aware that there is anything particularly wrong. Should a case come under treatment in

this early stage, I would strongly recommend that a trial should be given to the plan of *mechanically fixing the entire side* by one of the methods to be now described. In order to be of any use it should be done effectually, so as to restrain the movements as much as possible, and the sooner the application is made, the more likely is it to be of service. The plan I originally adopted was the following:—Strips of adhesive plaster, from four to five inches wide, were fixed at one end close to the spine, and then drawn tightly round the side as far as the middle line in front, the patient being directed to expire deeply. In this manner the whole side was included, commencing from below and proceeding upwards, each succeeding strip partially overlapping the one below. One was also fixed over the shoulder. Over this layer of plaster strips of bandage of the same width were fixed in like manner, having been previously dipped in a mixture of mucilage and chalk, such as is used in the treatment of fractures. Two or three layers of these were laid on, and then heated sand-bags applied, in order to dry the application as soon as possible. This is a most effectual mode of fixing one side of the chest, while it leaves the other quite free to act; and I would, by the way, commend it to those who are called upon to treat fractured ribs. The plaster adheres firmly to the skin, and the bandages adhere to the plaster, a firm casing being formed which will remain on any length of time. With regard to pleurisy, however, I have since then adopted another plan, which, so far as this disease is concerned, seems sufficiently efficacious. It is merely to use strips of plaster, putting on two or three layers in the following manner:—The first strip is laid on obliquely *in the direction of the ribs*, the second *across the course of the ribs*, the third in the direction of the first, about half overlapping it, the fourth as the second, and so on until the entire side is covered. A strip is also passed over the shoulder, which is kept down by another fixed round the side across its ends. Now it is difficult positively to prove that this treatment actually checks the course of pleurisy; but, taking a common-sense view of the matter, it is not improbable that such a result might be anticipated; and, from my own experience, I have not the slightest doubt but that it is brought about. I have carried it out now in a good number of

cases, and in all the course and termination have been most satisfactory, while relief to the pain and other distressing symptoms has been generally immediate. I feel convinced, also, that in many of those cases of extensive pleuritic effusion which come under observation, the accumulation might have been prevented or moderated had this plan of treatment been adopted at an early period.

In another set of cases of pleurisy there is found to be a moderate amount of effusion when the patient first comes under treatment. Here, too, I would recommend efficient fixing of the side. In those cases in which I have carried it out, I have almost always had satisfactory results, whereas I have more than once regretted the neglect of this plan of treatment. Where the effusion is very abundant, but little can be expected from it, though I think that occasionally it has appeared to aid absorption. Now and then cases present themselves in which there is extensive exudation of lymph over the surfaces of the pleura, with but little fluid. Here the only object to be desired is to bring about adhesion of the surfaces as soon as possible, and strapping the side firmly aids this most certainly. In bilateral pleurisy of course this mode of treatment can scarcely be practised; and where this disease complicates others it will be of less service.

*(To be continued.)*

## PLEURITIC EFFUSION WITH ACUTE MANIA CURED BY PARACENTESIS THORACIS.

BY RICHARD GREENE,

*Senior Assistant Medical Officer, Sussex Lunatic Asylum, Hayward's Heath.*

It is to be feared that paracentesis thoracis for the removal of fluid effused consequent on acute pleurisy is not yet so common as to prevent the following case being interesting to most readers of the *Practitioner*. It is still too much the custom to regard this simple operation as a *dernier ressort*, and not as the ordinary treatment for pleuritic effusion, *when* we feel satisfied that the fluid is not being rapidly absorbed, or when there is much disturbance of the circulation or of the respiratory movements. The longer the fluid is allowed to remain in the pleura, the greater is the danger of irremediable lung-mischief supervening; and the risk of the operation being very slight, it is certainly better to err on the safe side, performing it too soon rather than leaving it until the lung, or part of it, has become so carnified that resumption of its function is impossible.

Those old true-blue practitioners who object to make a tiny wound in the thoracic wall should remember that they never hesitate to ruin their patient's constitution by the "judicious use of mercury," drift him into phthisis by blood-letting, inflict horrible torture by repeated blistering, or drench him with nauseous drugs, named diuretics by courtesy.

No operation could have been undertaken under more unfavourable conditions than in the following case, and yet the result was perfectly satisfactory. The patient's disease had been mistaken by his medical attendant: he was exhausted

from want of nourishment and from a long railway journey, and he was in a state of raving madness, having no knowledge of his surroundings or of his own weakness. Moreover, having no Bowditch's syringe at hand, I had to use an ordinary trocar and canula, conducting the fluid under water by means of a few feet of indiarubber tubing. I am satisfied, however, that no air entered the pleural cavity.

A. B., aged 28 (No. on Register, 2,228), was admitted into this asylum on the 8th of March, 1873, suffering from acute mania. It was stated by the medical practitioner who certified to his insanity that he was then suffering from pneumonia, but that the thoracic disease had supervened on the insanity, and that they were not related to each other as cause and effect. On admission, mentally he was almost as ill as it was possible for him to have been, and his incoherent ravings were, as is so often the case, divided between perverted religious ideas and the most disgusting obscenities; while, physically, he was found to be in a very exhausted state. His pulse 136, weak and compressible, respirations 33, and temperature in the axilla  $100^{\circ}3$ . There was little or no respiratory murmur audible on the right side, but it was intensified on the left, and percussion elicited a dull sound over the whole of the right half of the thorax, which was also bulged slightly. It was at once seen that the case was not pneumonia, but pleurisy with effusion. Alcoholic stimulants were carefully administered, and under these the patient to a certain extent revived; but it became more and more evident that the only hope of recovery lay in paracentesis thoracis, which I consequently performed a few hours after admission. Four pints of fluid were removed, and the lung shortly began to expand. Six hours' sleep was obtained by the subcutaneous injection of one-sixth of a grain of morphia. The pulse, respirations, and temperature fell gradually till they reached the natural standard, and contemporaneously the mental state improved until reason was quite restored. A course of cod-liver oil and tonics completed the cure, and the patient was discharged on the 31st of May in all respects well and strong. A few weeks ago I heard from his relatives that he has preserved both his mental and bodily health, and that he is daily employed at his trade.



Such cases as these show the fallacy of regarding insanity purely as a speciality, or of treating it by remedies addressed to the brain only; and they also tend to prove that the mind, so far from being a distinct entity, is indeed "the most dependent of all bodily functions." Here the salvation of a man's reason depended upon the evacuation of a few pints of fluid from his chest.

## ON TISSUE-DESTRUCTION IN THE FEBRILE STATE, AND ITS RELATIONS TO TREATMENT.

BY DR. ANSTIE.

THE present paper is published under special circumstances, and can only be regarded as an instalment of what must hereafter become a very extensive inquiry. In the further series of observations with regard to the function of alcohol in pyrexia, which I am now carrying out, certain facts in the natural history of the pyrexial state have for the first time become known to me, and as they conflict in a considerable degree with the general statements of previous writers, I think it advisable at once to place them before the profession. It will be seen that they have very important bearings, not only on the pathology but on the treatment of pyrexia.

There is one maxim in pathology which physicians have for some time past been inclined to adopt without reserve, namely, that the persistence of the febrile state at a certain high standard and for any considerable number of days, involves of necessity a large tissue-consumption, and that this destructive process almost invariably involves, to a large extent, the muscular and other nitrogenised tissues of the body. The evidence of this destruction of nitrogen-containing tissues is readily to be found in the fact that, notwithstanding the diminution or even entire cessation of nitrogenous food-supplies, the urea-discharge is as high as, and usually very much higher than, in health. Besides, it is sufficiently obvious, in most cases of severe typhoid fever, pneumonia, and other intensely febrile affections, that great wasting of the muscular tissues takes place before the period of convalescence sets in.

Now the extended inquiry which I have been lately carrying on has shown, in a very surprising manner, the possibility of a total absence of urea-increase during the course of an intense pyrexia of considerable duration. That is the broad fact, in the natural history of the febrile state, which has come out: I now proceed to give details.

Before entering into the question now raised, it is needful to have distinct ideas of what constitutes a genuine urea-excess. The first thing necessary to know is, what is the average urea-discharge of health.

Now it is obvious, in the first place, that the amount of urea will vary enormously according to the character of the diet. It would be impossible otherwise to account for the exceedingly various estimates of the health-range of urea-discharge which have been given by excellent observers. In Parkes's work on the urine, twenty-four authors are quoted whose estimates were all made on males between the ages of twenty and forty; the figures vary from 286.1 to 688.4 grains for the twenty-four hours. The mean of the whole gives an estimate of 512.4 grains urea-discharge daily. But in Senator's recent and most valuable work on pyrexia<sup>1</sup> I find, with surprise, that he quotes a statement of Unruh, which would make the normal discharge in men as low as 270 grains (17.466 grammes) in twenty four hours. It is quite certain, however, that unless the individuals from whom this was taken were feeding on a diet unusually poor in nitrogen, some abnormal circumstance must have been present.

The second point to be remembered is that the concurrent results obtained by many observers show that in healthy adult women the daily urea-discharge does not exceed three-fourths of the quantity excreted by healthy adult men similarly circumstanced. According to our modern physiological ideas it would be easy to account for the whole of this difference by the inferior quantity of azotised food consumed by females.

The third point to be remembered is, that in health the urea-discharge is the result, in by far the largest extent, of the decomposition of nitrogenous food within the body.

<sup>1</sup> Untersuchungen über den fieberhaften Process, p. 95. Berlin: Hirschwald, 1873.

In the febrile state, on the contrary, there is usually such complete anorexia that the supplies of nitrogenous matter taken are very small, yet the urea-discharge is commonly far higher than in health ; sometimes double the amount, or more. Not only in case of a positive increase of urea, under these circumstances, but even when it sinks some way below the normal level, we may still conclude (from the small nitrogenous food supply) that exaggerated tissue destruction has largely to answer for its formation. The question is, can the urea-discharge be so far depressed below the normal rate, in any considerable number of cases of decided pyrexia, as to indicate decidedly that nitrogenous-tissue destruction is taking but little part in the actual urea-formation? The cases which will be brought forward in the present paper certainly tend, as far as they go, to answer that question in the affirmative.

(A.) *Typhoid Fever*.—In the case of this disease a large body of evidence has been collected, of late years, which appeared to show, with great uniformity, that when the fever was at all severe (as measured by continuously high temperatures, together with any marked delirium, the urea-discharge was constantly above the normal level, at a time when a very small amount of azotised food was being taken. For example, Dr. Parkes,<sup>1</sup> besides speaking very definitely, from his own experience, in favour of a regular increase (save in certain abnormal conditions) of urea beyond the normal level (one-fifth on the average), also quotes three important series of observations, by Moos, Brattler, and Warnecke, extending altogether over 78 cases. The significance of the figures will not be perceived unless we remember that the patients were on exceedingly low diet—lower than we should employ in England. Moos (from eighteen cases) gives the average daily discharge as 575·64 grains in first week, 517·92 grains in second week, 404·04 grains in third week, 343·2 grains in fourth week. Warnecke gives, from results of fifty cases (thirty males and twenty females), for *men* an average discharge of 673·92 grains in first week, 617·44 grains in second week, 482·04 grains in third week, and 361·92 in fourth week ; for *women*, 530·4 grains in first week, 471·12 in second week, 375·96 grains in third week, 319·8 grains in fourth week. Brattler gives (from ten cases) 607·4

<sup>1</sup> Op. cit. 1862, pp. 244 *et seq.*

grains daily in first week, 592·8 grains in second week, 421·2 grains in third week, 327·6 grains in fourth week, 247·6 grains in fifth week. The near approximation of these different series of results is very remarkable. Nevertheless, Parkes himself points out some objections to their conclusiveness; and my own results will be found to fully justify a revision of the usual belief. That is to say, they will show that the exceptions to the rule are probably so numerous as to form an important subject for consideration in regard to fever doctrines and fever treatment. The histories now to be given are of patients in whom neither stimulants, nor any important medicine, were given during the observed period.

The first case which I shall relate is that of Charles Jefferson, aged 25, a small but well-nourished man, who was admitted into Westminster Hospital on the seventh day of typhoid fever, and upon whom the first rose spot was seen on the following day. The examination of the urine unfortunately could not begin till the ninth day. At this time there was no diarrhœa: but I may mention here that even at the period when diarrhœa did come on we did not lose urine from that cause, as the patient intelligently obeyed our orders to pass water into the chamber-vessel before going to stool. This circumstance shows, of course, that the case was not of the severest type. Full convalescence was established upon the twenty-fourth day, as marked by the cessation of diarrhœa; the temperature had become normal two days earlier. I had intended to print in diagrammatic form the respective curves of mean daily temperature, of urea elimination, and of water elimination, but there was unfortunately no time for this on the present occasion,<sup>1</sup> so the figures must be verbally stated. On the ninth day the temperature, though not quite so high as it had been, was nearly 103° on the mean of three observations, the maximum being a good deal higher: in striking contrast with this was the fact that only 118 grains of urea were eliminated. During the five following days the temperature had steadily come down and the urea had as steadily gone up; the twenty-four hours' mean temperature on the

<sup>1</sup> All the facts connected with the course of fever treated with, and without, alcohol, will be subsequently published in diagrammatic form as well as in verbal description.



fifteenth day stood at 100·5, while the urea had risen to 385 grains. Here, unluckily, the patient became mutinous; for although he had diarrhoea, and slight delirium every night, he felt so well and hungry in the day-time that he was greatly disgusted with his rigid diet. I had kept him strictly upon milk alone—80 ounces daily; and except a very little lemonade, and the usual dilute hydrochloric acid mixture, nothing else was allowed to pass his lips. At this point he became so clamorous that he would probably have left the house had we not gratified him: but to do so with safety we had to make the diet so complex that it would have been impossible to estimate its value by means of analysis. Accordingly, we did not estimate the urea during the next three days; but so far as the volume (judged by the eye merely) of urine and its colour<sup>1</sup> could afford a hint, it did not seem likely that any very material change in the urea elimination had taken place during this time. After this three days' interval we resumed the diet of 80 ounces of milk only, and our first twenty-four hours' estimation gave only 316 grains urea. Next day (same diet) the urea fell to 284 grains: the mean temperature was now 99°·6. On the day following (twenty-first)) there was a sudden increase in the volume of the urine (from 550 c.c. to 2,000 c.c.), and the urea-reading was 717 grains: but herewith a remark is necessary. No corrections had been made, at any point in the case, for chloride of sodium; for on the one hand it was impossible for me to give the time for *exact* estimations of the chlorides (having a large number of urines of acute diseases to examine every day), and, on the other hand, the rough formula given by Liebig for chlorides becomes worse than useless where there may be great critical variations. Throughout the decidedly febrile period of Jefferson's illness there could be little doubt, from the very moderate level of the figures representing the total effect of the nitrate of mercury test, that the chlorides were in great part either retained or eliminated in the stools, as is well known to be usually the case. But on the cessation of fever there is usually a sudden and rather considerable appearance of chlorides in the urine; and part of the 717 grains that read as urea in

<sup>1</sup> Marked changes in urea-discharge are generally accompanied by distinct changes in discharge of colouring matters in acute pyrexia.

my table in all probability really represented Na Cl, especially as the diarrhœa was declining. Respecting the diarrhœa and its possible influence on the urea-discharge, it may be remarked, first, that during the early period (three days) of *lowest* urea elimination there was no diarrhœa, indeed no action of the bowels at all: and secondly, it has been long ago shown by Parkes that the diarrhœa of typhoid fever, though it does favour the escape of chlorides, is not to any considerable extent a channel for nitrogen-escape.

The volume of water steadily fell again (though no accurate measurements were taken) during the next two days. On the twenty-fourth day the volume was ascertained to be 1110 c.c. (39.6 ozs.), and the urea was (again without correction for chlorides) 383 grains. On the day following it was only 223 grains (also without correction for chlorides). At present he is on a mixed diet, approaching as fast as he safely can to the food of a healthy man. As soon as he shall have become fairly established on that, I will report on the amount of his normal urea-discharge. He is a hearty eater when in health.

The broad features of the above case are: (1) That the water elimination and the urea-discharge closely coincided in amount, but that they ran a *precisely opposite course to that of the mean temperature* during the whole period between the ninth and the fourteenth day. [It will be seen, hereafter, that even the water and the urea by no means always correspond in amount.] (2) That the average urea-level was exceedingly low, for it might well have been expected (according to former estimates) that such a patient would have excreted a daily average of 500 grains during the second week of typhoid, whereas he actually passed only 300 on the average. (3) I may now add that the nitrogenous matters introduced as food (in 80 ozs. milk) proved on analysis<sup>1</sup> to yield 113.5 grains nitrogen, or the equivalent of 227 grains urea: so that it will be seen that but a small portion of the urea-discharge needs to be accounted for by destruction of proteinous tissues. (4) In correspondence with the slight degree of muscular destruction which took place in this case was the remarkably small degree in which muscular

<sup>1</sup> The milk supplied to our hospital is good in quality. It is intermediate, *quâ* nitrogen, between that of St. George's and that of the Middlesex.

strength was affected. This was repeatedly pointed out by me to the students; and the same fact has been noted in several other cases.

Just before this case occurred, another patient, Mary Hughes (aged 32, single), had also been in Westminster Hospital, suffering from typhoid fever.<sup>1</sup> The past history of the case was very confused. It was impossible to say whether, at her admission, she was already in the fourth week of typhoid fever, whether she had had a "relapse," or whether she had passed into typhoid after suffering from some other febrile affection. At any rate she was in a condition of very high pyrexia, the temperature fluctuating between 104° and 105°, or even more for the first two days, and crops of well-marked rose spots appeared daily for five or six days after her entrance to the hospital. She had no diarrhoea at any time; but this is a feature which has been exceedingly common for some months past, even in some of the best-marked cases of typhoid at Westminster Hospital, and, I believe, elsewhere. She had no delirium, no appearance of anxiety, and although she lay in bed her muscular strength seemed scarcely at all affected. It was curious to see this in a patient the dry burning heat of whose skin would have led one to expect a very profound degree of prostration. From January 24th to February 2nd inclusive (nine days), the total nitrogen of the urine was daily most carefully estimated by my friend Mr. Frederick Hicks (formerly assistant to Professor Sir Benjamin Brodie), by the soda-lime combustion process as modified by Parkes. On several of these days I myself examined, for urea only, by Liebig's method; and the difference between our figures was only so large as corresponded well enough with the probable amount of nitrogen contained in the excreted lithates, uric acid, creatine, creatinine, &c. The diet was rigidly confined to 60 oz. milk (containing a little over 85 grains nitrogen) during the first two days, for the other seven it consisted of 40 oz. milk, and 20 oz. best beef-tea (containing nearly 120 grains nitrogen). The nitrogen discharge<sup>1</sup> was as follows:—

<sup>1</sup> Under the care of my colleague, Dr. Sturges.

<sup>2</sup> Decimals are omitted. If the nitrogen be, for convenience, reckoned *all as urea*, these figures will have to be doubled, to express the whole as an urea-amount.

105 gr., 115 gr., 149 gr., 103 gr., 87 gr., 116 gr., 77 gr., 44 gr., 170 gr. As it has been suggested to me by one or two friends that cases with a low urea-discharge may perhaps void a large amount of nitrogen through the skin, I may remark that at any rate this patient did not perspire *fluid* at all considerably, nor did the man Jefferson.

The daily volume of water eliminated by the woman Hughes corresponded somewhat, but not closely, with the fluctuations of the urea. The figures (in cubic centimetres) are: 512·5, 570, 590, 500, 710, 635, 700, 700, 980. From this it can only be said in general terms that both urea and water were low; but it will be seen that, *e.g.*, while on the seventh and eighth days of observation the volume of water was exactly the same, the nitrogen was 77 grains on the former and 44 on the latter day; and many other discrepancies could be pointed out. The above nine days were the only ones on which the total nitrogen was estimated; but for three or four days longer (I should mention that temperature became normal two days after the last combustion-research) I watched the urine, by Liebig's process, on the chance of a great rush of urea and water elimination, but nothing of the sort occurred. The urea slowly rose up to what appeared (on mixed diet) to be about the woman's normal discharge, viz. 350 grains. This being probably her standard in health, it is very remarkable to observe the low ratio of elimination during the nine days first mentioned, and especially during the first four of them, when the temperature was *never below* 103° at any part of the twenty-four hours. During these four days the urea-discharge did not average more than 238 grains; while the ingoing nitrogen averaged 102·5 grains, equivalent to 205 grains of urea. And it is interesting to observe that this evidence of a but slight destruction of the azotised tissues is fully in agreement with the woman's remarkable conservation of muscular power.

It will immediately occur to some, as a criticism on the above case, that the urea-discharge might well be low, seeing that the patient had been continuously or nearly continuously febrile for a month when our observations began, and the available albumen of the blood and tissues might have been used up by a copious urea-discharge in earlier stages of the disease. But



it seems difficult to suppose that anything of the kind had taken place; for in such case there must have been extreme muscular weakness, whereas the very reverse was the fact. And indeed, supposing that we regard the above as a case of typhoid really at the end of the fourth week at the time of admission, the best evidence hitherto obtained (including Parkes's) goes to show that when the fever keeps up at this late stage the urea-discharge keeps up also. The remark may also be made, in anticipation of a similar criticism on Jefferson's case (in which the observations were not commenced till the ninth day), that his entire freedom from muscular prostration was inconsistent with the idea that there had been any such enormous combustion during the first week as might have temporarily used up the nitrogenous matters available for elimination. Since writing the above I have analysed Jefferson's urine, first day of *full diet*:—Chlorides normal; urea, 503·88 grains.

Before carrying further this enumeration of cases with high temperature and low urea-discharge (which will be resumed in the next number of this journal), I wish to point out the present critical state of the questions upon which must depend the principles, which may ultimately be agreed upon, for the treatment of the pyrexial state, in order that the readers of this journal may perceive that the present investigation—though apparently concerned only with the clinical history of febrile diseases—has a practical bearing on the treatment of a number of acute diseases. But my remarks on this subject will find their most natural and relevant expression in the shape of a review (deferred, for want of space, to the April number) of Senator's work on the Febrile Process, which I would beg my readers to peruse before studying the continuation of the present paper in our next issue.

(*To be continued.*)

[REVIEWS, CORRESPONDENCE, and BIBLIOGRAPHY are postponed by the pressure on our space.—*Ed. Pract.*]



## Clinic of the Month.

**Transfusion of Blood in Anæmia.**—Dr. Barnes observes that there is nothing more sad than to see a woman in labour or childbed bleed to death. It is surely, he remarks, a just reproach to the medical art if such a catastrophe occur. The first thing to study is how to obviate the causes of hæmorrhage; the second is how to put a stop to hæmorrhage when actually present and threatening to continue; the third is how to rally the system from dangerous exhaustion induced by the loss of blood.

The highest achievement of medical science would undoubtedly be to secure the first of these three ends, and thus to exclude all necessity for considering the remaining two. Unfortunately we cannot in all cases command all the essential conditions. We must therefore make up our minds how to deal with hæmorrhage and its effects. The number of cases in which hæmorrhage cannot be averted, now that we have so effective a resource in rebellious cases as the topical use of perchloride of iron, is becoming more limited every day. Still, even in some cases where hæmorrhage has been stopped, life may ebb away unless restorative means be applied. The greatest of these is transfusion, and Dr. Barnes rejoices to see the revival of this operation concurrently with the extending use of the perchloride of iron.

The following case, although not successful, is not without interest in several points:—A short time ago Dr. Barnes met Dr. Devereux, of Tewkesbury, at a case of extreme exhaustion from secondary post-partum hæmorrhage. At one time it was thought the patient was dead, so utter was the prostration. The pulse rallied now and then, but often flagged so that it became imperceptible; the respiration was laborious and frequent, so loud that it could be heard in the adjoining room; the face and extremities were cold; the voice almost extinct. She swallowed beef-tea and port from time to time, but soon vomited all. This was about 8.30 P.M. The extreme prostration, and the hopelessness of getting any nutriment absorbed either by stomach or bowel, led us to look upon transfusion as the last hope. Discussing the means of accomplishing this, we found the best we

could do was to use an aspirator-syringe. This had to be fetched at a distance of five miles. In the meantime, the gardener offering his arm, at 10 P.M. the operation was carried out. The syringe being made on the principle of the stomach-pump, with reversible action, answered extremely well. The point of the aspirator-trocar was filed down, and this served very fairly for insertion into the patient's vein. As circumstances left no choice between immediate and mediate transfusion, it became necessary to defibrinate the blood. The man who yielded the blood was fat, and his veins small, so that it was with difficulty four ounces were obtained. This was defibrinated by whipping with a silver fork and filtering through a cambric handkerchief. Believing that one factor in the danger attending these cases of prostration is the merely dynamic one arising from the absence from the heart and vessels of fluid to act upon, and considering that he had so small a quantity of blood at his disposal, Dr. Barnes first filled the syringe with a solution of phosphate of soda, carbonate of soda, and chloride of sodium, at a temperature of 100° Fahr. Pumping this through the syringe also served to get rid of air from the apparatus. The vein in the patient's right arm was found by pinching up a fold of skin, transfixing it transversely to the course of the vein, and then dissecting through a little fatty cellular tissue. It was then pinched up by forceps, and a sufficient opening made into it. The trocar was passed one inch and a half into it. The injection was then slowly made. The blood was partly mixed with the saline solution; altogether about six ounces of fluid, including all the blood, was thrown in. Almost instantly the pulse improved, warmth spread over the body, the face and manner became more natural, the respiration became tranquil, and hope was entertained that she might pull through. Everything was prepared for repetition of the transfusion in the morning should the condition be favourable. But the rally was short; the respiration again became moaning, rapid, and laboured; the pulse flagged, and the patient sank at 1.30 A.M., about three hours after the injection. (*Lancet*, Jan. 3, 1874.)

**Treatment of Diphtheria with Calomel and Soda.**—Dr. Duer, of Philadelphia, extols the efficacy of small doses of calomel and large doses of the bicarbonate of soda, and the free use of nutritious food and brandy, in diphtheria. He states that he has treated a large number of cases of all grades of severity by this plan with satisfactory results, which he says was first suggested to him by Dr. Harlow. Two cases are related to illustrate this mode of treatment. Grace V., aged five, previously strong and well, after short prodromic symptoms and a marked chill, presented all the general and local evidences of diphtheria. Her

sister Alberta, aged seven, was taken sick the same morning, and presented an almost identical condition. The former had ten grains of chlorate of potash every third hour, as recommended by Vögel, and the other one-eighth of a grain of calomel and five grains of bicarbonate of soda. A weak solution of carbolic acid as a disinfectant was applied locally, and the same supporting and stimulating plan was adopted in both cases. The following morning there was little change in either case. On the evening of the second day, however, there was the most marked improvement in Alberta, while Grace's symptoms showed little evidence of yielding. Dr. Duer now withdrew the calomel and continued the soda. By this time, in this case, the false membrane was coming away in detached fragments; but in the other child there had as yet been no change for the better. On the evening of the fourth day, Alberta's tonsils were again covered with false membrane; calomel was again given for twenty-four hours, with the same positive result. From this time the soda was depended on entirely. At no time was there the slightest pytalism, but as soon as the child had been long enough under the influence of the calomel, the false membrane seemed to yield up its connection with the tonsils, while at the same time the general symptoms began rapidly to abate. On the morning of the fifth day the temperature had fallen to 99° and the pulse to 90, and from that time she steadily improved under the use of tonics and nutritious diet. On the other hand, Grace, to whom the chlorate of potash had been given, continued so ill that Dr. Duer substituted the calomel and soda treatment on the fourth day, with a like immediate result, so far as the local trouble was concerned; but her convalescence was tedious, and was subsequently complicated with bilateral paralysis of the palate and lower limbs. In the course of a few weeks Dr. Duer treated five other children in the family by the same plan, with complete success. (*British Medical Journal*, Jan. 3, 1874.)

**Treatment of Syphilitic Affections of the Nervous System.**—Dr. Broadbent, in his Lettsonian Lectures on this subject, remarks that the one remedy is iodide of potassium; or, this failing, mercury. He usually begins with doses of six grains, and always combine with it ammonia—the carbonate or aromatic spirit. Having by one or two days' experience ascertained that there is no special intolerance of the iodide, it may rapidly be pushed to doses of twelve, eighteen, twenty-four, thirty, or thirty-six grains, three times a day; occasionally, even larger doses are necessary, and he has given a drachm every four hours. That large doses are often absolutely required, and that they succeed when moderate doses fail, he is convinced by abundant experience: and if iodism is induced, which is very

rarely the case in tertiary syphilis, it is almost always before large doses are reached. Large doses are better borne when taken after meals. Of course iodide of potassium is more quickly taken up into the blood from an empty stomach; but it is also quickly taken out of the blood and in the urine; and when a continuous action on the system is needed, which is what we require in dealing with the effects of tertiary syphilis, the indication is best met by giving so diffusible a remedy as the iodide of potassium after food. If the iodide of potassium fails after a full and free trial, a resort to mercury is always desirable, and the more acute the syphilis the earlier. When passing from the use of one drug to the other, either a certain interval should be allowed to elapse, or the mercury, if given by the mouth, should be in one of its most soluble and active forms, the bichloride or biniodide. Occasionally, sudden and profuse salivation will occur when this precaution has been neglected, no doubt from the mercury being converted into biniodide within the system. Sometimes mercurial inunction may be employed at the same time with internal administration of iodide of potassium; and Dr. Broadbent says that frequently he has given biniodide of mercury with iodide of potassium, either in the same mixture or in the form of pill at night. One word as to the *modus operandi* of iodide of potassium. This was the subject of a beautiful explanation by Dr. Odling, in his Gulstonian Lectures before the College of Physicians, hypothetical at that time, but demonstrated by experiment since. The active agent is the iodine, as shown by the fact that other salts of potassium have not the same effect, while other combinations of iodine, such as iodide of ammonium or sodium, have. The iodine is permitted to exercise its influence on the seat of disease in virtue of the comparatively slight affinity by which it is held in union with the base, this being so feeble that in the presence of certain forms of living protoplasms in active change, the salt is decomposed, and the iodine set free to exercise its solvent action on the organic matters. Whether this is direct or indirect, through the well-known oxidising effects of free iodine, is not so certain. (*Lancet*, Feb. 21, 1874.)

**Treatment of Cerebro-spinal Meningitis.**—Dr. Dowse, of the Central London Sick Asylum, after giving a good account of the etiology, symptoms, and post-mortem appearances of this disease as it affects the base of the brain, observes that there is no disease requiring more constant watching or careful medical interference than this. He has seen an acute meningitis and myelitis treated with those drugs which produce congestion; for instance, opium and strychnine. Nothing can be productive



of more harm than this administration in the first or acute stage.

1. It has to be considered how to relieve the vessels of the cord and to equalise the action of the vaso-motor system of nerves. Nothing appears to be of greater service in effecting this than the ergot of rye and belladonna. The former he has prescribed in decided doses, such as half a drachm of the powder every four hours; and the latter he has applied to the spine in the form of a belladonna paste, made by mixing the extract with one-third its weight of glycerine.

2. To check the reflex vomiting, small pieces of ice must be swallowed, not sucked, as the full effect of its sedative influence upon the stomach is then attained.

3. To relieve constipation, Dr. Dowse prefers the administration of a pill of the watery extract of aloes, for the reason that it acts upon the mucous membrane of the rectum and dilates the hæmorrhoidal veins.

4. To relieve sleeplessness, both chloral and bromide of potassium have proved ineffectual; but what he found of most service was a suppository of eight grains of the extract of henbane, with four of the extract of conium.

5. One essentially practical point must not be forgotten—namely, to keep the paralysed bladder constantly free from urine. It is not sufficient to draw off the water night and morning, which is the course usually adopted, but a self-retaining catheter must be kept continually in the viscera.

6. In reference to diet, it ought to be both nutritive and stimulant from the first.

7. There is a stage in the treatment of this disease when quinine in large doses becomes of the most signal value—at that crisis when exhaustion appears imminent; the skin covered with sweat; sudamina and bullæ over the body; temperature  $102^{\circ}$  to  $105^{\circ}$ ; pulse small, weak, and over 120. But more especially is quinine invaluable when rigors supervene; it never fails to have a good effect. But it must be given in ten or even twenty grain doses: and if the stomach cannot tolerate it, it must be introduced into the system by the rectum.

8. The detraction of blood, either local or general, is not advisable. (*Medical Times and Gazette*, Feb. 7, 1874.)

**Treatment of Diabetes.**—Dr. Brunton, after giving a full account of the pathology of diabetes as far as our present knowledge of it extends, makes the following observations on the treatment of diabetes. The patient must be supplied with a diet consisting of nitrogenous food, such as butcher's-meat, fish, eggs, and soups. Fat (which does not contribute in the least to the formation of sugar) may be given in all its forms, such as



cream, butter, cheese, and oil. Spinach, lettuce, and cresses may be freely used, but celery and radishes only sparingly; while potatoes, carrots, parsnips, turnips, peas, French beans, cabbage, Brussels sprouts, cauliflower, brocoli, asparagus, sea-kale, and fruit of all kinds, both fresh and preserved, should be avoided, with the exception of nuts and almonds. Instead of bread, the patient should take either the gluten-bread supplied by Bonthron, 106, Regent Street, and Van Abbot, 5, Princes Street, Cavendish Square, or the bran- or almond-biscuit prepared by Blatchley, 362, Oxford Street. Dr. W. Richardson strongly recommends that the change from an ordinary to a restricted diet should be made very gradually, lest the patient become disgusted with his food. Rather than produce this injurious effect, it is better to relax the diet and permit him to eat sparingly of bread made of whole meal, or even of white bread toasted and potatoes. In the cases of diabetes which depends on imperfect glycogenesis, the restricted diet will be sufficient to prevent the appearance of sugar in the urine. Should it still continue notwithstanding the adoption of this regimen, the circulation in the liver must be reduced as much as possible. For this purpose, the blood-pressure should be reduced, and the blood should be drawn to the surface of the body by warm clothing and warm baths. The Turkish bath should be used occasionally. Bleeding is not likely to be employed now as a means of lowering the blood-pressure, but Lefèvre records a case in which diabetes was cured by its use conjointly with that of warm baths.

It is very difficult to determine the point at which the irritation is situated on which reflex dilatation of the hepatic vessels may depend, and even if we could localise it, we might be unable to remove it. We should therefore direct our attention rather to the nervous centres, through which the irritation is reflected to the liver; and, by lessening their excitability, try to diminish its power over the hepatic vessels. The two remedies which are most serviceable for this purpose are opium and its alkaloid, codeia. Bromide of potassium and atropia, which might be expected to be useful, have been found of no service by Kretschy and Duchek. Half a grain of opium may be given three times a day to begin with, and the dose gradually increased. Codeia, an excellent remedy, which we owe to Dr. Pavy, may be given in doses of a quarter to half a grain three times a day at first.

Excellent results have been obtained by Kratschmer from the use of morphia, the sugar disappearing completely from the urine, and the nutrition of the patient being greatly improved. Harley recommends conia or cannabis indica, and has seen great benefit derived from a combination of conium, cannabis, and hydrocyanic acid.

In certain cases, quinine proves extremely serviceable; and,

whenever there is a history of exposure to malaria, it ought to be tried. Blumenthal narrates a case of diabetes occurring in a man frequently troubled with migraine, and of a very nervous temperament. The sugar was slightly lessened by a meat diet and the use of Carlsbad water, but the benefit was but slight, and the disease resisted codeia, arsenic, lactic acid, tannin, iron, and glycerine. Under the use of eleven and a half to thirty grains of quinine daily, the quantity of urine diminished; its specific gravity decreased; the thirst became less troublesome; the albumen, which had previously occasionally appeared in small quantities, entirely disappeared; and the nervous affections rapidly improved.

Several years ago Salkowsky discovered that the livers of animals poisoned by arsenic contain no glycogen, and that it is impossible to produce diabetes in such animals either by puncture of the fourth ventricle or by curare. He therefore proposed arsenic as a remedy in diabetes. From this recommendation, Leube gave it to the extent of one-third of a grain daily with good effect.

Alkalies were proposed as a remedy for diabetes by Mialhe nearly thirty years ago, on the supposition that they would accelerate the decomposition and combustion of sugar in the organism. They frequently do prove very beneficial, but it is not certain that they increase the combustion of sugar. It is quite possible that they do so; but, at any rate, the experiments of Lomikowsky seem to show that they lessen the production of sugar by diminishing the quantity, or preventing the action, of the diastatic ferment by which glycogen is transformed into sugar. This author finds that, when bicarbonate of soda is given to dogs for some time, little or no sugar is found in their livers, even when they have lain for several hours after death. They contain glycogen, and therefore the absence of sugar must be due to want of ferment. Livers taken from other healthy dogs, which had got no alkalies, always contain much sugar when allowed to die in the same way. Lomikowsky's conclusion is confirmed by the experiment of Pavy, who found that the previous injection of carbonate of soda into the circulation prevented the appearance of sugar in the urine after removal of the superior cervical ganglion. A convenient way of giving alkalies is in the form of Vichy and Carlsbad waters. These seem to be more beneficial when the patient visits the springs than when he drinks them at home, probably because he is thus induced to take exercise. The Vichy waters are purely alkaline, but the Carlsbad waters are purgative also, and are therefore to be preferred when any tendency to constipation exists. When the destruction of sugar is defective, we may endeavour both to increase it and to supply easily combustible materials to the

body in place of those products of the decomposition of sugar which normally serve as fuel to the organism.

Several months ago, Dr. Brunton attempted to increase the decomposition of sugar in diabetics by supplying the ferment which he supposed to be wanting. Since sugar is probably decomposed chiefly in the muscles, the ferment which splits it up is probably contained to a much greater extent in them than in any other part of the body. By giving the patients raw meat, we may hope that the ferment contained in it will be absorbed from the intestine into the blood, and there act on the sugar. It is necessary that the meat be given raw, for the heat to which meat is exposed in cooking completely destroys all ferments. In order to increase the oxidation, iron may be administered; and Dr. Brunton finds a combination of perchloride of iron with hydrochlorate of morphia and spirit of chloroform very satisfactory.

Lactic acid may be administered in doses of from fifteen or twenty minims to a drachm; but, instead of giving it alone, Dr. Brunton very much prefers to give it in the form of lactate of soda, as a double benefit would be thus obtained. The organic acid is burnt off when combined with soda, and the salt is converted in the blood into a carbonate; and in this way we get the benefit which is often obtained by an alkaline treatment. The form in which he always recommends it, however, is that of butter-milk, as this quenches the thirst, supplies food, and also contains a ferment which, if absorbed, may be useful in aiding the conversion of sugar into lactic acid within the body. He is inclined to attribute the benefits occasionally derived from the use of skimmed milk to its possessing similar properties to butter-milk; but he considers the latter superior. When it is allowed to become very sour, and all its milk-sugar has been converted into lactic acid before it is used, it would probably be still better.

Glycerine is strongly recommended by Schultzen as a substance which readily undergoes combustion in the body. He finds that, when a patient is placed on meat diet, the sugar disappears from the urine, and the thirst is no longer felt; but the nutrition remains indifferent. When glycerine is given at the same time, the muscular weakness diminishes, and the patient rapidly improves. He gives about half a fluid ounce to a fluid ounce and a quarter by measure (20 to 50 grammes by weight) of glycerine, with about 80 grains (5 grammes) of tartaric or citric acid in rather less than four pints of water daily. The patient drinks it from time to time instead of plain water. If the quantity of glycerine be increased to about an ounce and a half (60 grammes), there are sometimes nausea and diarrhœa. If the patient be not very thirsty, the glycerine may be given in

one half or one quarter the quantity of water. If Schultzen's theory of the decomposition of sugar be correct, a combination of glycerine or lactic acid or some butter-milk along with a meat diet ought to yield very satisfactory results. Pavy found that glycerine increased the quantity of sugar passed by a patient, and made him thirsty. He gave it in doses of ten ounces a day, and the unsatisfactory result he obtained is probably due to the quantity being so large; for the glycerine, being more easily combustible than the sugar, appears to have prevented its decomposition, and thus allowed it to accumulate in the blood. This case of Pavy's indicates to us that where diabetes depends on the formation of sugar being increased without its combustion being diminished, we need not expect to benefit our patients by supplying them with easily combustible foods, such as lactic acid and glycerine. We need, therefore, hardly try these remedies, except when the temperature of the body is low, as ascertained by the thermometer, or by the feelings of the patients, who complain of cold, and like to be constantly near the fire or covered with warm clothing. (*British Med. Journal*, No. 686, 1874.)

**Treatment of Granular Lids.**—In a clinical lecture on this subject, Dr. Swanzy remarks that the first and most important thing is to provide abundance of fresh air, both within doors and without. The patient should never be allowed to remain moping in the house, as he is apt to do, but should be made to take several hours' open-air exercise daily. More, he is convinced, may be done in many conjunctival diseases by fresh air alone than by any other treatment without it. It probably acts directly and locally on the conjunctiva, and not in any round-about way through the constitution. When vascular reaction is insufficient for the absorption of the granulations, it should be excited; when excessive, it should be restrained. Hyperæmia may be excited by warm fomentations and by sulphate of copper. Excessive blennorrhœa may be checked by nitrate of silver solution, containing ten grains to the ounce, applied by means of a camel-hair brush to the completely everted upper lid. A little solution of common salt should be at hand to remove excess, and this again may be washed away with plain water. The effect can be modified by the length of time the solution of the nitrate is allowed to remain in contact with the membrane. When the blennorrhœa is only slightly in excess, the liq. plumbi subacet. dil. of the Pharmacopœia without spirit is an admirable thing; it also should be washed off with plain water, and its use in this way is not contra-indicated by the presence of ulcers on the cornea. It is most important in using any local application to thoroughly evert the upper lid, in order



that that part of the membrane may be reached which is reflected from the lid to the globe, for a neglect of this part may render the treatment abortive. Fresh cases of acute granular ophthalmia (military or Egyptian ophthalmia) do not require any topical application. Ice compresses alone may be placed on the lids, a leech or two at the inner canthus, and the patient should be purged. (*Irish Hospital Gazette*, Jan. 15, 1874.)

**Apollinaris Water.**—A company has been formed, with offices at 19, Regent Street, for the purpose of importing the valuable effervescent mineral water known as the Apollinaris Water. This water is the product of a spring in the valley of the Ahr, not far from the Rhine, and has qualities intermediate between those of the Seltersbrunnen of Nassau and the Krähnchen at Ems. It is perhaps the best natural mineral water for table purposes, and far excels the ordinary manufactured aerated waters, especially in this respect,—that after withdrawing a portion of the contents of a bottle, it may be corked and set aside for even twenty-four hours, after which it will be found brisk as ever, instead of being flat and vapid like ordinary seltzer. Apollinaris water contains a considerable proportion of sodium salts, mainly carbonate, chloride, and sulphate, and will from these probably be found to act much in the same way as Ems water does in catarrhal affections of mucous membrane, for which that water is so renowned. But speaking merely from the lower standpoint, we think that Apollinaris water is certain, when better known in this country, to take a high place as a beverage. (*Medical Times and Gazette*, Feb. 7, 1874.)



## Extracts from British and Foreign Journals.

**The Physiological and Therapeutic Action of Emetine.**—Dr. A. E. d'Ornellas, in a paper on emetine, arrives at the following conclusions:—1. Emetine is the active principle of ipecacuanha, to which its powerful physiological and therapeutic action is due. 2. The fat odorous nauseous principle is but an accessory. 3. In medical practice emetine should not be substituted for ipecacuanha, as it is not so perfect a medicament. 4. Emetine has an irritating topical action upon the mucous membrane, and upon denuded flesh, but not upon the intact skin. 5. Emetine injected hypodermically into animals or human subjects induces vomiting, as when administered by the stomach, but more slowly and in larger doses. 6. Emetine introduced into the circulation is always eliminated by the gastro-intestinal mucous and liver, and it causes vomiting up to the moment of its elimination. 7. Emetine during its elimination by the intestinal mucous membrane causes stools more numerous in proportion as its emetic action has been less energetic. 8. In moderate doses, emetine diminishes the respiration and, slightly, the circulation; it lowers the animal temperature, but does not change directly the vascular tension. In emetic or nauseating doses the action is the same, but more energetic. 9. Emetine has an anti-convulsivant action, and by the relaxation it produces in the voluntary muscles combats effectively the convulsions caused by carbolic acid or strychnia. (*Pharmaceutical Journal*, Jan. 17, 1874.)

**On Subcutaneous Injections of Iodine in acutely inflamed Tissues.**—Dr. A. Menzel states that parenchymatous injections of various substances have recently been adopted in neoplastic and hyperplastic formations and in chronic inflammations, and recommends that similar measures can be advantageously adopted in acute inflammation. A very fatal epidemic of diphtheria in Trieste, which resisted all ordinary remedial means, afforded him frequent opportunities of

trying this plan. In this epidemic, glandular swellings occurred in the neck with great rapidity and of enormous size, and he endeavoured to counteract these conditions by the injection of iodine: the fluid injected was composed of potassium iodide, 5 parts; iodine, 1 part; distilled water, 1,800 parts. Of this four drops were injected into the tonsils, the arches of the palate, and into the tissue of the soft palate. He observed seven cases in which this plan proved very successful, and in no instance were any injurious consequences noticed. Dr. Menzel recommends the trial of iodine, carbolic acid, quinine, bromide of potassium, and ergotin, in erysipelas and ordinary phlegmonous inflammation. (*Wiener Wochenschrift*, 1873, 45, and *Der Praktische Arzt*, No. 11, 1873.)

**Fatal Result of Injection into the Vagina.**—M. Lorain records an interesting observation of an injection into the vagina being followed by death. The patient was a young girl, sixteen years of age, who was suffering from vaginitis, probably of a blennorrhagic character. After an emollient treatment, consisting of baths and injections of decoction of marsh-mallow, as the surface of the vagina could not be painted with a brush filled with solution of nitrate of silver, M. Lorain gave directions that a weak solution of the nitrate should be injected. This was accordingly done at 10.30 in the morning, with a small syringe containing scarcely five centigrammes of the fluid. The quantity of the solution injected only contained one decigramme (1.54 grain) of the nitrate, and it was thrown in with due care. Violent pain was immediately experienced. The patient displayed the most marked agitation. Ice was applied over the belly and to the vagina. The temperature of the vagina was 37.8 C. The following day the temperature fell to 37.2, but vomiting occurred. On the three succeeding days the condition remained unchanged. Stomatitis then supervened, resembling closely that produced by mercury. On the fifth day after the injection death occurred suddenly at 7 P.M. M. Lorain attributes the accident to the penetration into the peritoneum of a few drops of the pus proceeding from the passages and Fallopian tubes consequent on the writhings produced by the pain. A post-mortem examination made by M. Tardieu showed that there was metritis with suppuration. The *tubæ* were filled with pus, some of which had flowed into the peritoneal cavity and produced a diffused peritonitis. M. Lorain cites a number of other cases in which a similar accident had happened. He thinks that every woman attacked with vaginitis, accompanied by symptoms showing that the ovaries and Fallopian tubes are affected, is in great danger. The care of the practitioner should be directed to the prevention of all

excitement on the part of the patient. Pain should be allayed by opiates and by hypodermic injections, the bowels kept open by means of emollient injections and sitz baths, whilst the abdominal walls should be supported and rendered immovable by a corset of elastic collodion; and, finally, no examinations should be made beyond those that are absolutely required for the purposes of diagnosis. (*Bulletin Général de Thérapeutique*, liv. ii., 1874.)

**The superior value of Artificial Premature Labour, and Turning over the High Forceps Operation and Craniotomy in cases of Contracted Pelvis.**—A paper with this heading was recently read by Dr. Alexander Milne before the Obstetrical Society of Edinburgh, in answer to a previous paper by Dr. Macdonald. Dr. Macdonald had maintained that in contracted pelvis delay should at first be tried, and if that did not succeed the forceps should be tried; if this instrument failed, resort should then be had to craniotomy. Further, Dr. Macdonald thought that the operation of inducing premature labour in cases of contracted pelvis was to be discouraged and even abandoned. In reply to these views, Dr. Milne points out in his paper that they are founded, in part at least, on very unfavourable statistics obtained in Germany, and then takes up the subject of premature labour. This, he maintains, as far as British practice is concerned, is essentially a safe operation. If a single woman is killed by it, the fault must attach to the operator, not to the operation; and as regards the child, the late Professor Hamilton saved 42 out of 46 deliveries. Dr. Milne gives a series of cases where the operation had succeeded in his hands under unfavourable circumstances (contracted pelvis). Next, in regard to craniotomy, Dr. Milne states he has performed it frequently, but confesses he has a waxing sense of the responsibility attaching to the operator. He thinks that every possible resource should be exhausted before plunging in the steel; admitting that five mothers die in 100 after the operation, how badly does this contrast with the results of artificial premature labour! The next alternative is the forceps; not, he observes, the beautifully conservative short forceps, but the long forceps, or rather the high operation; that is to say, the head is lying above or at the brim, and fails, owing to pelvic contraction, to descend. It will not come down without great force, necessitating great compression of the cranium. This fact alone condemns the high operation, for the forceps ought not to be a compressor, but only a tractor. As regards the child, it often produces idiotcy, whilst the maternal risks must be patent to all. It is requisite, for instance, in high cases, to use the forceps within the os uteri, and there appears to be a widespread con-

viction of its dangers among experienced men. Dr. Milne concludes by reiterating his opinion of the value of premature labour. (*Edin. Med. Journal*, February 1874.)

**Pathology of the Sympathetic Nerve.**—Dr. A. Otto records a case in which a previously healthy woman suffered from vomiting, headache, and fainting. In the course of fourteen days the left half of the face, neck, and chest exhibited a strongly marked redness. Amblyopia, loss of consciousness, difficulty of speech, occasional feeling of fulness in the head, with well-marked delirium and gradually increasing depression of spirits, were successively noticed. Dr. Otto concluded from these symptoms that there was disease of the left sympathetic, and determined to treat it with the constant current, the cathode being applied to the cervical sympathetic. After the very first sitting, the patient ceased to feel vertigo, the erythematous redness disappeared, and the visual power, speech, and mental activity underwent improvement. The two latter symptoms were referred by Dr. Otto to persistent relaxation of the cerebral vessels in consequence of the paralysis of the sympathetic, which, as no other cause could be discovered, he attributed to rheumatism. The physical conditions causing the amblyopia were not, unfortunately, examined, but he is inclined to think neuro-retinitis was present. There was no contraction of the pupil of the affected side. A symptom that, however, was observed, and has not hitherto been described, was a difficulty of breathing, occurring especially at night, and compelling the patient to leave her bed. (*Deutsch Archiv für Klin. Med.* 1873, xi.)

**Ditaine, a succedaneum for Quinine.**—A plant was shown at Vienna during the late Exhibition, the *Echises scholuris*, belonging to the Apocynaceæ, which may probably come to be used instead of quinine. It appears that several plants belonging to the Apocynaceæ are employed for the same purpose as quinine in the Philippine Islands. The present substance is amorphous, bitter, and highly hygroscopic. If found to be equally effective it possesses a great advantage over quinine in its cheapness, as two pounds by weight would only cost 5*l.* (*L'Union Médicale*, No. 983.)

**Influence of Antiseptic Substances on the Virus of Septicæmia.**—At a recent meeting of the Society of Biology in Paris, M. Dumontpallier read, in the name of M. Davaine, a paper on this subject. He stated as the result of his experiments that certain substances in very small relative proportions are capable of neutralising the septicæmic ferment. Thus the addition of  $\frac{1}{1000}$  of carbolic



acid,  $\frac{1}{15000}$  of sulphuric acid,  $\frac{1}{30000}$  of caustic potash, and the same amount of chromic acid, or of  $\frac{1}{100000}$  of iodine, completely destroys the fermentative power of blood to which they had been severally added. In some remarks that were made after the reading of the paper, M. Onimus observed that he had several years ago brought similar facts under the notice of the society, and that he had particularly showed that although the septicæmic properties of the fluid were abolished, the bacteria and vibrios still retained their vitality.

**Excision of the Knee-joint.**—In a paper on this subject, recording a successful case of this operation, Mr. Hayes, of the Mater Misericordiæ Hospital, Dublin, gives the following as the conclusions at which he has arrived in regard to the operation:—

1. Excision of the knee is, in its immediate effects, a less severe operation than amputation through the thigh.
2. Recovery after excision is tedious, and taxes the constitution far more than does healing of an amputation wound.
3. Excision cannot be recommended during the progress of rapid disease of the knee-joint, especially if attended with suppuration, when that disease has no assignable origin other than constitutional defect.
4. Progressive disease of the soft textures of the knee, and even of the articulating bony surfaces, need not forbid excision, provided the general health is good, and that the disease can be traced to some cause not inherent to the constitution.
5. Should all active disease have subsided, whether originally of constitutional or other origin, and the patient's health be sound, the surgeon may prefer excision to amputation if the object of his operation is to render the limb useful to the patient.
6. Union occurs firstly in the soft textures, then, through the medium of granulation tissue, between the bones; but at least four months, and more frequently from six to eight months, must elapse before that union can become sufficiently firm and reliable for the surgeon to pronounce upon the result of his operation.
7. Bone may be freely removed when, owing to disease, the proceeding becomes necessary; but it is better to use the gouge than the saw in such cases, as it is essential for good union that the diameters of the opposed surfaces should be ample.
8. He esteems it a matter of importance that the surgeon should remove not only all trace of synovial membrane and crude formations—the sequences of inflammatory action—but also every portion of articular cartilage which may remain after the application of the saw. Lastly, he adds that, whilst he would advise members of the middle and upper classes of society (whose means would enable them to obtain well-constructed artificial limbs, and whose avocations do not demand constant daily exercise) to undergo amputation in preference to



excision of the knee, he considers the latter operation, in suitable cases, likely to afford a far more valuable result to the poor townsman and peasant, who must depend upon bodily labour for the means of support. (*Irish Hospital Gazette*, Jan. 1, 1874.)

**The use of the Elastic Ligature in Surgery.**—The employment of the elastic ligature, as recommended by Mr. Dittel, has become very general. The case which led to the discovery of its powers was a remarkable one. A little girl, eleven years of age, somewhat wild, was compelled by her mother-in-law to wear on her head for fifteen days continuously a band for her hair, containing a thread of caoutchouc. On admission into the hospital she complained of headache, and on examination a deep groove was found running round her head, filled and obscured by a fleshy out-growth at the bottom of the groove, and beneath the new growth was the thread of caoutchouc. It was removed, but in fifteen days the child died of meningitis. At the *post mortem* it was ascertained that the thread had divided the hairy scalp and the bones. Throughout nearly three-fourths of the bone affected the whole thickness had been cut through, and beneath this the dura mater had mortified. Estimating correctly from this singular event the extraordinary power of constriction capable of being executed by a thread of caoutchouc, Mr. Dittel thought it ought to be applied to the removal of pedunculate tumours, and accordingly tried it, using for this purpose a piece of a drainage-tube of small diameter. He also applied it for the removal of an erectile tumour, placing it beneath some needles which were passed through the tumour. Then he extended its use to all kinds of operations, even to the performance of amputations. The mode of using it is very simple. A passage is made with a trocar, and the caoutchouc passed through the canula. The two ends are then tied tightly together, and the section is gradually effected. This kind of ligature can be advantageously used to cut through portions of skin bridging over fistulous tracts near the anus. Cases of prolapse of the rectum, of phymosis, and all kinds of tumours have thus been treated with success. In amputations effected by this means, it is of course hardly necessary to say the bones must be divided with a saw. It is an easy mode of performing castration. Elastic ligatures have been employed for the closure of arteries, and quite recently M. Grandesso-Silvestri has used them to effect union in a case of Cæsarian operation. (*Journal de Médecine*, January 1874.)

**Cotton-wool Dressing.**—The following advantages are claimed by M. Verneuil for the cotton-wool dressing which was largely employed in the Franco-Prussian war:—(1) Subtraction of the wound from the incessant action of the air, and from the

dangers of absorption of deleterious principles from it; (2) regular extended, persistent, and methodically applied compression, modifying the afflux of blood, and preventing congestion taking place towards the wound; (3) constancy of temperature; (4) avoidance of slight secondary injuries; (5) perfect rest of the region wounded; (6) the establishment of a peculiar and special condition on the surface of the wound. The wound should be covered in all cases with a thick layer of wool, in order to suppress pain and to protect it from miasms. Around the wound a bandage should be lightly rolled, and then should be tightened on the following day. In the course of a few days the wool and bandage begin to exhale a disagreeable odour; but there is no danger, as absorption does not take place. (*Le Mouvement Médical*, Jan. 10, 1874.)

**Trimethylamine or Propylamine in the treatment of Acute Articular Rheumatism.**—M. Peltier, after giving the details of 118 cases treated by means of trimethylamine, states, *en résumé*, that 22 cases were cured in less than eight days; 81 in from eight to fifteen days; 7 within thirty days; and 8 appeared to be but little influenced by the remedy. He concludes therefore: (1) That from the point of view of its chemical action, it is preferable to make use of the chlorhydrate of trimethylamine. (2) That from a physiological point of view trimethylamine is a slight excitant for the skin, a caustic for the mucous membrane, a sedative for the nervous system, a hyposthenic for the arterial system; and lastly, it appears to diminish the amount of urea in the urine. (3) From a therapeutical point of view trimethylamine has only been employed in acute articular rheumatism; it calms pain, it reduces congestion, when present, in the articulation, and it diminishes fever. Finally, it appears to be the remedy which up to the present time has given the best results. (*Le Progrès Médical*, Jan. 10, 1874.)

**Comparison between Hysterotomy and Ovariectomy.**—In a clinical lecture delivered at the Hôtel-Dieu by M. Richet, the professor drew a comparison between hysterotomy and ovariectomy. He pointed out that there was no relation between them in regard to their mortality; that in hysterotomy it was necessary to make the opening sufficiently large to permit the whole tumour to escape, whereas in ovariectomy, when no adhesions are present, a very small opening is all that is requisite to allow the cyst to be emptied and its collapsed walls to be drawn out. In hysterotomy the incision must often extend from the pubis to the xyphoid cartilage. Hence the extremely dangerous nature of the operation. To diminish this danger it has been proposed to cut the uterus away piece by

piece; but there is then the danger of fatal hæmorrhage. Secondly, when the opening into the abdomen is made and the tumour has been removed, an extensive sutured wound remains, and the chances of evisceration are great, and it has actually happened in M. Riche's practice; and although in this instance the patient really died from pyæmia, the danger is not less real. The chief difficulty of the operation of hysterotomy is that of pediculating the tumour. In ovariectomy the walls of the cyst are often thin, and after the evacuation of the liquid a ligature can easily be applied; on the contrary, in hysterotomy the pedicle is often so large that it is often requisite to divide it into two parts, and ligature each half separately. It is necessary to tie the ligatures sufficiently tight to prevent hæmorrhage; at the same time care must be taken not to lacerate the tissues, which are often sufficiently friable. After the ligature has been applied it remains to draw the pedicle to the surface, but the pedicle is inextensible, and in fact is directly continuous with the uterus. Hence for some time after the operation the patient experiences severe dragging pains in the pelvis, and sometimes it is absolutely necessary to relax the ligature and to remove the clamp. In ovariectomy it is possible to cause complete adhesion between the peritoneum and the pedicle; in hysterotomy this is not possible, and then a wall is formed from which it is requisite to remove the purulent contents with the utmost care by frequent washing. (*Le Progrès Médical*, Jan. 10, 1874.)

**Treatment of Otorrhœa.**—The pathology of the auditory apparatus, says M. Ménière, is in general too much neglected by those who practise general medicine and surgery; and in a large number of cases, even when perfectly simple and uncomplicated, the practitioner is uncertain what plan of treatment should be recommended. Even in cases where it cannot be expected that the treatment should equal that of the specialist, who is rapid in his diagnosis and expert in the use of instruments, it is yet the duty of every surgeon to take care that the affliction should not be allowed to become incurable, and to give some alleviation to the sufferings of the patient. Take, for example, the case of discharge from the ears, so frequent in childhood and youth. What is the procedure often adopted? They are left alone to run their own way, in fear lest the sudden suppression of the discharge should injure the general health of the child; or some injections are tried, often very imperfectly accomplished, and if the discharge still continues no further trouble is taken till the infirmity has become incurable. M. Ménière cites a case of a tall, fair, but not scrofulous, youth, twenty years of age, who was sent to him, and who at the age of seven or eight had been

attacked with discharge from the ears after some eruptive fever. No systematic treatment had been adopted for him, but occasionally injections had been practised. He passed his childhood, and at eighteen came to Paris. The discharge continued, and steady increase of deafness was observed. On examination M. Ménière found that serious lesions of the auditory apparatus were present. The membrana tympani was destroyed, vegetations had sprouted from the walls of the tympanic cavity, and there was a purulent discharge. Appropriate treatment being commenced, great improvement resulted, but, of course, without change in the irreparable damage that had taken place, and which might certainly have been prevented had proper care been expended upon him while a child.

In all cases of otorrhœa great attention must be paid to the constitution, so that scrofula, syphilis, or other constitutional disease should be treated by appropriate general measures. In this lies an essential element of success in all instances. Systematic injections play an important part: they cannot do harm, and they are almost certainly productive of immense advantages. Cleanliness is a capital point in the treatment of otorrhœa, and nothing is better for this purpose than pure warm water injected from an ordinary syringe with moderate force, the nozzle being placed fairly within the meatus. The caoutchouc pears may be used, but the stream they give is less continuous and strong than that from a syringe. In the early stage, and when the otorrhœa is accompanied by sharp pain, the treatment is but little different. A good injection is composed of warm decoction of marsh-mallow, in which one or two poppy-heads have been boiled; this may also be poured into the affected ear, the patient resting his head on the sound side. A leech or two may also be applied behind the ear, the second being allowed to attach itself to the same point seized by the first. The whole ear may be covered with a poultice of linseed meal on which a little laudanum has been sprinkled. M. Giampietre recommends as a topical application the instillation into the meatus of two or three drops of a liquid containing one-sixth of a grain of aconitina in one ounce of distilled water. M. Ménière rejects the instillation of laudanum, ether, or chloroform. He objects also to the instillation of oil of almonds, and other similar fluids, so commonly employed: he thinks they often serve to aggravate the original evil. Where the pain is very intense he adopts the plan of subcutaneous injections of morphia, &c. Otorrhœa of old standing is more frequently complained of by patients than acute attacks: and in their treatment warm injections are always indicated. The fluid injected may either be pure water or a very weak solution of alum, one to five grains in two ounces. Solutions of sulphate of zinc and acetate of lead may



also be used of the same strength. No other treatment will effect improvement, if injections, which remove pus and the secretions of the meatus, are neglected. A little piece of wool dipped in a weak solution of carbolic acid may be placed in the orifice of the meatus after each injection: a little weak solution of nitrate of silver may be employed in the same way, and may also be injected once a day, the ear having been first thoroughly cleansed by the injection of warm water, and dried by the subsequent introduction of a little warm dry wool. Neither of these topical applications, and especially of carbolised glycerine, is painful or harsh, as they simply cause a tickling sensation in the ear, and the secreting surface is thus modified without harm. M. Ménière frequently uses the following lotion, the ear having been previously injected with water and dried:—

Water, 200 parts.

Glycerine, 100 parts.

Sulphate of zinc, 5 to 6 parts.

Another lotion, which may be used even when there is great vascularity at the bottom of the meatus, and even in cases of perforation of the tympanum, is—

Acetate of lead, 5 to 15 parts.

Water, 300 parts.

In both cases a few drops may be allowed to remain in the ear for eight or ten minutes. By the use of these means it is not to be expected that every case of otorrhœa will be cured, but at all events the disease will be prevented from getting indefinitely worse, and the patient placed under the most favourable conditions for special treatment. (*Journal de Médecine*, Art. 9630.)

**Hemiopic and Sector-like Defects in the Field of Vision.**—In the last part of M. Brown-Séquard's *Archives of Medicine*, Dr. Knapp, of New York, read an important paper on hemiopic defects in the field of vision, which he shows may be consequent either on diseases of the inner coats of the eye, or may depend on morbid conditions of other parts of the body, especially the heart and the brain. They hence become of interest to physicians not engaged in the culture of a specialty. The simplest method of examining the visual field, and one which is sufficient for almost all practical purposes, is the following:—Let the patient cover one eye, and incline his head toward the side of the covered eye and a little upward; the surgeon should take a corresponding attitude, about  $1\frac{1}{2}$  feet before him, so that if the patient closes his right eye the surgeon closes his left, and both look steadily at each other's eyes. If a small white ball on a black stick be moved



in various directions in a vertical plane midway between the two, the surgeon will be able to compare his visual field with that of his patient, and perceive at once any deviation from it. The *regular defects* in the field of vision may occur in one eye only or in both, and may be either hemiopic or sector-like. Dr. Knapp points out that attacks of hemiopia may arise (1) from embolism of one main branch of the arteria centralis retinæ; whilst sector-like defects are due to embolism of secondary or tertiary branches, the symptoms being sudden impairment of vision, with partial atrophy of the disc and attenuation of the obstructed arteries, which have marginal white striæ, and disappear at a short distance from the optic papilla; (2) from compression of a main branch of the central retinal artery, by exudation in neuro-retinitis, consequent on brain disease, of which he gives a very interesting case. (*Brown-Séguard and Séguin's Archives of Medicine*, No. 4.)

## Department of Public Health.

### QUARANTINE IN RELATION TO CHOLERA.

DR. GAVIN MILROY has again brought the subject of quarantine before the profession. Recent experiences of the practice of quarantine in the West Indies, and the further light thrown upon it by the wide and varied application of quarantine on the continent of Europe during the recent epidemic of cholera, have led him to think that the question might very usefully be subjected again, at the present moment, to professional discussion. He initiated such a discussion at the February meeting of the Epidemiological Society, by submitting the following propositions :—

“1. As quarantine is a practical question, its value or otherwise can only be determined by the results of experience, independently of theoretical considerations. In respect of cholera, there has already been ample experience acquired in this and other countries, during the successive European epidemics since 1831, to test its value both by sea and land.

“2. In 1865 the Council of this Society declared their opinion to the then President of the Privy Council, that quarantine, as enforced in many continental countries and in our own colonies, afforded no trustworthy protection against the invasion of cholera, while it served to create false expectations of defence, and to foster neglect of internal sanitary precautions.

“3. In 1866 an International Conference was held in Constantinople. The conclusions of the Conference in relation to quarantine, adopted by a majority of the members, have never been accepted by the most experienced men in this country. They are quite at variance with the opinions expressed by the Council of this Society in the previous year.

“4. The detailed history of the outbreak of the disease in 1866 in a single one of the West India islands—viz., in the French island Guadeloupe,—and of the circumstances which preceded the outbreak, and of the different conjectures respecting the supposed importation of the disease by a vessel from France, shows how inexact and misleading is the evidence that is too often accepted concerning the origin of epidemic occurrences.

"5. The existing state of quarantine legislation and quarantine practice in our own West India islands is extremely faulty, and urgently demands revision. It well deserves consideration by the Council of this Society, whether the attention of the Secretary of State for the Colonies should not be directed to this important subject of State medicine.

"6. The quarantine order of our own Government, issued last summer in respect of the precautionary measures to be adopted towards infected or suspected arrivals in our ports, might form the basis of sound legislative enactments on the subject in all our colonies. The order in question will be regarded by continental upholders of quarantine as a virtual condemnation of the system approved of by them.

"7. The more thoroughly that the subject of quarantine in relation to cholera is investigated, the more exact and instructive will be the information we acquire touching various points in the natural history of that pestilence which are still obscure and uncertain."

It cannot be said that the revival of this question is premature. Indeed, Dr. Milroy had been anticipated by the International Medical Congress which met at Vienna last year, and which had devoted a considerable portion of its time to the subject. The discussion at the Epidemiological Society was unexpectedly abortive. Curiously enough, in the first place, it went completely astray, and after it had been with some difficulty brought back, by the president, to the right track, a perverse disposition of the various speakers to deal with the incidental rather than the essential portions of the propositions, marred the discussion. The first case was instructive and deserves prominent notice as illustrating a growing vice in professional phraseology. The speaker who led off the discussion, Inspector-General Murray, M.D., late of the Indian Medical Service, used the term "quarantine" as including all measures adopted for the exclusion of infectious diseases from healthy communities, whether living on the sea-coast or inland; and he illustrated at some length the benefits derived from measures taken for the protection of gaols and isolated communities in India against cholera. Readers of Indian sanitary literature will have noted that this mode of using the word is not uncommon with Indian medical men. Thus, for example, we have a notable instance in Dr. Cunningham's recent report of the cholera epidemic of 1872 in Northern India (see Sections 91, 95, 96, &c.) In England also this word has latterly been coming much into use, particularly colloquially, in this sense. Practitioners now not uncommonly speak of the act of isolating patients ill of an infectious malady as

putting them in quarantine. We lately heard one medical man tell a mother that her child, attacked with scarlet fever, would have to be placed in quarantine several weeks, meaning thereby that it would have to be confined to the house for that period; and another medical man inform a friend, as to a family having several members affected with measles, that the house should be quarantined for some time, meaning thereby that it would be well not to hold personal communication with the family for a few weeks. The evil arising from this growing loose use of a defined term was very aptly illustrated at the discussion of the Epidemiological Society; for the vague and inaccurate use of the word quarantine at the commencement of the discussion caused a loss of time which could not be recovered.

*Quarantine* is a technical term having a precise well-understood definition, and it is desirable that it should be limited to this its proper meaning. Quarantine, the noun, according to the last edition of Webster, signifies—"specifically, the term, originally of forty days, but now of undetermined length, during which a ship arriving in port, and suspected of being infected with a malignant, contagious disease, is obliged to forbear all intercourse with the shore; hence, restraint or inhibition of intercourse to which a ship is subjected, on the presumption that she may be infected." *Quarantine*, the verb, according to the same authority, signifies—"to prohibit from intercourse with the shore, to compel to remain at a distance, as a ship from shore when suspected of having contagious disease." It was in the senses here given that Dr. Milroy used the term, as is obvious from his propositions; and it is in this sense only that a discussion on "quarantine" can be of any practical value. Words (the "idols of the market") are sufficiently troublesome without our going out of the way to make them more so by needless laxity.

The gist of the propositions really rested in the first one, and it was greatly to be regretted that the discussion had not been limited to such illustrations as were to be derived from the experience afforded during the recent extension of cholera on the Continent. The reference to the outbreak of cholera in Guadeloupe during 1866 was unfortunate. This outbreak is in reality worthless as bearing upon the question of discussion. From the

measures successfully taken by the local Government at the beginning of the outbreak to suppress all accurate knowledge as to the nature of the disease, the time was lost when its early history should be inquired into with any reasonable chance of success. The consequence is, that the evidence recovered after the nature of the malady could no longer be concealed is valueless, *as standing by itself*, either in proof or disproof of the importation of the disease. But this outbreak is just one of those instances which afford unlimited scope for the ingenuity of partisans of opposite schools of thought. To modify a phrase of the inimitable Tristram Shandy, in the discussion "*de Nasis*" of the learned Slawkenbergius, "words are in proportion to the want of true knowledge."

We propose to offer a few observations on the general question of quarantine in relation to cholera, based chiefly upon the experience of 1873. The first information of an onward movement of cholera on the continent of Europe last year occurred in the month of May. On the 10th of May quarantine was declared by the ports on the Lower Danube against the Upper Danube; and on the same date it was declared upon the Bosphorus against the Danube and the Bulgarian ports of the Black Sea. It is now known that the activity of the disease on the Upper Danube commenced in February, and there can be little doubt that it had been disseminated somewhat widely along the course of the river and in Bulgaria before quarantine measures had been taken. Moreover, so far as the Bosphorus was concerned, the passengers travelling along the routes on the north bank of the river, by way of Odessa, remained free from any imposition of quarantine to the end of the year's extension of cholera. The first intimation of a movement of cholera in Western Europe was given in a telegram in the *Times* of the 3rd of June, which announced the appearance of the disease in Gumbinnen, East Prussia. It is now known that the disease had begun to descend the courses of the Vistula and the Elbe about three weeks earlier, and so rapid was its first extension, that on the 21st of June it had reached Hamburg. *But the presence of the disease in Hamburg was not known to the outer world until the first week in August.* About the same time that cholera had begun markedly to spread along the Danube and the lines



of the Vistula and Elbe, it appeared also in Northern Italy, having passed across the frontier from Hungary very early in the year, or late in the previous year; and long before a question of quarantine arose in Italian ports the disease had been widely disseminated in the north-east provinces. Genoa, Naples, and other ports were all attacked from inland.

Other instances of a like kind could be mentioned, but the above will be sufficient to show how, in the present state of traffic on the Continent, even with the aid of the telegraph, any hope of excluding cholera from any part of its sea-board by quarantine, when the disease exists in Central Europe, must be futile. They also indicate that in the face of migratory cholera from Egypt by way of the Mediterranean, the most dangerous period of extension, as happened in 1866, would most probably, nay, almost certainly, precede the imposition of quarantine in any of the ports of South Europe. The more closely the facts of the extension of cholera on the Continent last year are studied, the more clearly it will be apparent that a quarantine which aims at excluding cholera from a European port must in the vast majority of instances, if not in every instance, necessarily fail. Every line of railway laid down in Europe makes quarantine for the purpose of excluding cholera—cholera being already present in Europe—an anachronism.

So far as England is concerned this truth has now long been admitted, and it must be most gratifying to Dr. Milroy to find what good fruit his earlier labours as to quarantine have borne in this country. The experience of the past year fully confirms the justness of the policy which has been adopted by the English Government in respect to quarantine on these shores. The different introductions of cholera into London, Southampton, and Liverpool last year, having regard to our intimate communications with the Continent, could only have been anticipated by an absolute cessation of all commercial relations with the Continent, commencing from the beginning of the year. But even supposing, for the sake of argument, that such an impossible measure were possible, it would give no real security. For, as Mr. Simon pointed out, smuggling, the complement of trade restriction at sea, would spring up, and it was just as easy to smuggle cholera as to smuggle goods.

In brief, all experience tends to the conclusion that a quarantine, in Europe at least, which aims at more than stopping manifest cases of cholera or choleraic diarrhoea, and so supplementing internal measures of precaution, must of necessity fail.

There would appear to be some hope that this, the English view of the question, in view of last year's experience, is beginning to make way among professional men on the Continent. M. Léon Colin has advanced a very distinct step in this direction in his recent article on "Quarantine" (*Dic. Encyclop. des Sciences Médicales*). A still further step was taken in the resolutions of the International Medical Congress of last year. The presidents were Dr. Abdullah Bey, of Constantinople; Dr. Tomaschich, of Trieste; Dr. Witlacil, of Vienna; and Professor Caminhoa, of Rio de Janeiro. The following resolutions were adopted by the Congress almost unanimously:—

"1. The practice of quarantine as now carried out ought not to be maintained, because on the one hand it does not constitute a real protection (*une protection suffisante*) against contagious maladies; and on the other hand it is directly opposed to the interests of commerce and industry.

"2. Quarantine ought to be limited to the time requisite for the examination and disinfection of the ship, the crew, and the passengers; and if there be no disease on board, the latter should be released immediately after disinfection. But if there be cholera, or sickness of a doubtful nature on board, it will be necessary to isolate and disinfect the ship also. The regulations in respect to yellow fever and plague should remain for the present as they are.

"3. Existing quarantine regulations should be modified in accordance with Resolution 2.

"4. A permanent Epidemiological Commission should be formed, for the purpose of studying fundamentally and systematically all maladies arising from infection."

The altered standpoint of the foregoing resolutions is certainly full of promise, if the question of quarantine, as would appear to be probable, should shortly again occupy the attention of an International Conference specially appointed for the purpose. It was intimated to the meeting of the Epidemiological Society that such a Conference might be called together in the course of the year, as a consequence of action taken by the International Medical Congress, and the Society in consequence postponed for the time taking any steps upon Dr. Milroy's resolutions.

Quarantine in its present form cannot, we believe, be much longer maintained on the continent of Europe. The chief

danger to Europe now from cholera is from within rather than from without. Assuming that the nature of the traffic across the Isthmus of Suez is such as to admit of something like an effective quarantine being maintained against the extension of cholera from the Hedjaz or from India by way of the Red Sea, this does not represent the principal line of migration by which cholera travels. The route through Persia and Eastern Europe is still the favoured route. The International Sanitary Conference of 1866 believed that a system of quarantine could be established on the Caspian, and of observation on the Russo-Persian frontier, which would as effectually shut cholera out from Europe in this direction, as the system of quarantine and observation now established at Suez is believed to shut cholera out in that direction. Whatever opinion may be entertained of the effects of the measures at Suez and on the Red Sea, it is sufficient to state of the routes across the Caspian and Russo-Persian frontier, that the Russian Government has affirmed that precautionary measures there with a view to exclusion of the disease must as a mere matter of practice be futile. Moreover, Russian medical opinion, further, avers that for all practical purposes cholera must be held as having to some extent become naturalised in that country. If on the one hand Russia cannot protect herself against the extension of cholera from the East, and on the other cholera has made itself a home, or persists for indefinite periods in that vast country, the danger to Europe is infinitely greater from her inland lines of traffic than from the sea. In face of the facts of the recent diffusion of cholera in Europe, the prominence given to quarantine by almost all the continental countries having a sea-board was simply absurd. Note for example the exquisite folly of the measures of quarantine adopted in some of the Italian ports, these ports having become infected from inland. The railway has made any extensive recurrence to inland measures of exclusion impossible, and quarantine as a measure of exclusion must equally fall before the extension of the same agency, if its fall be not precipitated by better counsels among the different European Powers.

A foremost step to these better counsels must be a clearer knowledge of the subject among medical men. Now, Medicine in this matter is continually stumbling over a fallacy. It

confounds a logical sequence with a practical sequence. Logically, no doubt, cholera ought to be and should be prevented entering a community ; practically, forty years' experience has shown that it is impossible to close all the avenues of entrance for the disease. But it does not follow therefrom that quarantine is useless. Quarantine as a method of exclusion fails ; but quarantine as a means of diminishing the amount of the imported evil may be of great utility. The experience of 1866 and of 1873 teaches us, however, that the failure of quarantine in respect to cholera is not merely a failure of means taken to a particular end, but a failure very largely dependent upon the nature of the disease. In both years the extension of cholera on the sea-coast of Europe, in all important cases, preceded the possibility of establishing any measure of quarantine against the infection. These extensions, indeed, teach us that quarantine against cholera, if it is to be practised as a method of exclusion with any reasonable chance of success, should in fact be practised virtually in permanence. But even then, from the fallibility of human nature, it could not give us the desired security.

Quarantine, indeed, as a rule, however carried out, can never act otherwise than as a *disease-filter*, and from the moment that medical men throughout the world will begin to understand this, it will be possible to secure those modifications of the practice which will retain all the good it is capable of giving, and remove the most if not all the evils it more commonly inflicts.

## THE ETIOLOGY OF TYPHUS.

THE late protracted discussion in the Academy of Medicine on the etiology of typhus would have found a place in our "budget of paradoxes" but for the contributions to it, recently published, of Dr. Guillemin and Dr. Fauvel. The former, although travelling over the whole ground of the etiology of the disease, gives from personal observation an account of typhus in Metz in 1870, which disposes of the



paradoxical proposition that gave rise to the discussion (*Gazette Hebdom.*, Jan. 9, 1874). The latter took occasion of the discussion to relate the facts observed in the allied armies, as to typhus, during the Crimean War, also with regard to the typhus of the Tartar emigration from the Crimea of 1860, and the typhus of the Circassian emigration of 1863-64, the last-named outbreak being one of the most awful disasters of modern times. The facts of the disease during the two emigrations are little known in this country, and Dr. Fauvel has done good service by placing them on record (*Gazette Médicale d'Orient*, April and September, 1873).

The discussion originated in certain propositions submitted to the Academy by Dr. Chauffard, to the effect that typhus was not indigenous to France; that it never appeared upon French soil or among a French community except as a result of importation; and that even when imported it never became acclimatised. Typhus, in fact, according to M. Chauffard, is not a disease of the French race, and has only an accidental part in its pathology. These propositions were mainly founded on the asserted absence of typhus among the beleaguered populations and garrisons of Paris and Metz. We now know from M. Guillemin and others that typhus existed in Metz, and from the former gentleman we learn also the reasons why the disease never obtained formidable proportions among either the civil population or the garrison. It is highly probable, moreover, that what was true of Metz was true also of Paris. Of the last-named city Dr. Gordon has observed, in his *Lessons on Hygiene and Surgery from the Franco-Prussian War*: "Of pure typhus there has, according to the published statistics, been none throughout the siege; but, as observed by some medical men, there was a marked liability present among the wounded to fall into a condition in many respects resembling that of patients suffering from that disease, to which condition is attributable a considerable amount of the mortality among them. There is, indeed, much reason to believe that cases of pure typhus which did occur, instead of being shown separately in the returns, have been included among the typhoid; and it may be fairly doubted if, during the continuance of the siege, the strict line of diagnosis between the forms of disease was



drawn as it usually is in England, and doubtless would have been in Paris under normal conditions." (P. 235.)

## TYPHUS IN METZ, 1870.

Of Metz, Dr. Guillemin states that, contrary to the opinion commonly entertained, typhus did occur in the city during the blockade, concurrently with enteric fever. Upon this question the physicians practising there were almost unanimous, and some of them who had formerly been in the army had studied the disease during the Crimean War. What was observed in Metz was an example of a still-born epidemic (*épidémie de typhus avortée*). The disease never became general, never underwent the extension or assumed the gravity observed in the Crimea or in Algeria, for the sufficient reason that the causes giving rise to it had not been either so long in action or so intense. It prevailed most among the inhabitants of the suburbs who, at the commencement of hostilities, had fled into the city. This class of persons suffered, as a necessity of their position, from insufficiency of food and from overcrowding. Although the soldiery were not altogether exempt from typhus, it is incontestable that they suffered from the disease much less than the civil population; and it is to the slight incidence of the disease upon the troops that is owing the fact that not a few military medical men may not have observed it. Cases of petechial typhus were, however, recorded in several of the military ambulances by Drs. Herpin, Onzaneau, Rosman, Méry, and others, and at one time grave fears were entertained lest the malady should spread actively. Dr. Méry, writing on the subject observes: "I had charge of a young mobile, in the Rue des Trinitaires, who after being treated ten days in the Tabacs hospital, was brought home by his parents, and died of typhus as well characterised as the disease I saw in the Crimea. As to the sick in the city, I can assure you that I have seen among them numerous cases of typhus, some slight, some severe. I saw there the disease pursuing the same course that it followed on its apparition in the Crimea in 1854-55, and if the blockade had continued longer, we should have had a second edition of the Crimean disaster." Dr. Guillemin, himself, observed in

Metz several cases of slight typhus, but no case of petechial typhus. In his notes made at the time he wrote: "The typhus observed in Metz had not the characters of Oriental typhus. The fever and excitement at the beginning and the encephalic congestion were less considerable; the eruption was rare, little marked, but it was the eruption of typhus, not of typhoid. There was no intestinal complication, little or no diarrhoea, no tenderness in the right iliac region. The duration was short, rarely fifteen days in the cases observed by me. The convalescence was marked, quick, and sudden. Between morning and evening the sick would pass from a state of sickness to a state of convalescence. They asked for food, and they ate without danger."

The most clearly marked cases of exanthematic typhus, and the gravest cases, occurred, as already noted, among the poorest portion of the civil population. Some of these cases occurred also among the ambulances, especially in those ambulances in which the more serious forms of disease were treated. But, as a rule, the cases of typhus observed in both ambulances and hospitals were cases of "typhus abortif." These facts are readily explained. The poorer classes of the civil population, especially refugees, suffered much more from deficiency of food and from overcrowding than the wounded and sick soldiery. These latter were fairly nourished even to the end of the blockade. The diseases which prevailed in the army were neither very numerous nor very serious, and the number of cachectics were few. There was no great overcrowding either in the ambulances or hospitals. The feeding of the soldiers during the last fortnight or three weeks of the blockade had been insufficient and little varied, but then only. The predisposing causes of epidemics of typhus had not, in fact, exercised any action upon the army except for a very short time and with slight intensity. The state of things among the troops at Metz was in no respect comparable to that which existed among the army in the Crimea when typhus began to spread among it, or among the indigenous population of Algeria in 1863, when typhus broke out there. Add, finally, that the troops were not exposed to much fatigue.

## MR. STANSFELD'S PUBLIC HEALTH LEGACY.

MR. STANSFELD, in vacating the Presidency of the Local Government Board on the dissolution of the Liberal Ministry, has left a legacy of difficulties to his successor, to local authorities, and probably also, in time to come, to the Legislature, the magnitude of which even thus early it is not difficult to estimate. In the one question alone of appointment of Medical Officers of Health Mr. Stansfeld has contrived to stultify one of the most important aims of the Public Health Act 1872, and add to existing incongruities of sanitary districts, other incongruities more glaring than any which previously existed. It seems desirable at the present moment to set forth a plain recital of some of the results which have followed Mr. Stansfeld's public health policy. To this end we have analysed the accessible returns of appointments of Medical Officers of Health, and we give the results for the consideration of our readers. Let it be premised that the general inspectors of the Local Government Board, under the instructions of Mr. Stansfeld, advised differently as to the fittest arrangements for a Medical Officer of Health, at different times. In some sanitary districts it was advised that the Poor-law Medical Officers should be appointed Medical Officers of Health, each in his own district. In other districts it was advised that the Poor-law Medical Officers should *not* be appointed Medical Officers of Health, but that sanitary districts should combine so as to have a Medical Officer of Health in common, giving him a salary sufficiently large to secure his whole time. In still other districts it was advised that a combination of the two preceding forms of advice should be acted upon, but this third more advanced form of advice does not appear to have been adopted anywhere. These different kinds of advice were tendered without any reference to peculiarities of locality; they were offered simply as outcomes of official wisdom at different stages of development. Upon the advice thus proffered, Local Authorities set to work, particularly in rural sanitary districts, in conjunction as a rule with the Inspectors of the Local Government Board, to carry it into effect, and the results of this

combined Central and Local wisdom we have now before us in numerous instances. These will be best dealt with in four classes, to wit: (1) Selected districts, for the most part curiosities of appointment; (2) Districts with several Medical Officers of Health; (3) Districts in combination having one Medical Officer of Health; (4) Districts not in combination having one Medical Officer of Health.

1. *Selected Sanitary Districts.*—Of these districts there is a luxury of choice. Adlington, urban, appoints a Medical Officer of Health for a population of 2,606, located on 1,046 acres, and gives him 7*l.* a year. Alresford, rural, divides its districts into two parts, to each of which it appoints a Medical Officer of Health. The first part has an acreage of 17,000 and a population of 3,734; the second part, an acreage of 22,000 and a population of 5,530; and to each Medical Officer a like salary is given, namely, 10*l.* Ashby-de-la-Zouch, urban, with an acreage of 4,505 and a population of 4,110, pays the Medical Officer of Health 10*l.* 10*s.* a year. Aston, Warwick, rural, having an acreage of 27,385 and a population of 17,544, pays the Medical Officer of Health a salary of 150*l.* Baldersby, urban, enjoys a Medical Officer of Health, for a population of 296 and an area of 1,744 acres, at a salary of 3*l.* 3*s.* Bangor, urban, with a population of 7,733, with a clearer eye to economy, gives its Medical Officer of Health “fees for services rendered.” Basford, rural, stands out an almost typical district, for with an acreage of 53,326 and a population of 81,561, it takes a Medical Officer of Health’s whole time, and gives him 500*l.* a year. Chard, rural, also gives a probably fitting stipend (for whole time or not is uncertain) of 350*l.* for an acreage of 60,342 and a population of 28,852. Conssett, urban, on the other hand, with an acreage of 10,824 and a population of 6,000, thinks that every useful purpose is met by giving a Medical Officer of Health “1*l.* 1*s.* per day when he is required.” Hartlepool, urban, holds that 10*l.* a year is a sufficient salary for a health-officer’s care of 13,000 people; and Hull, with an obvious fellow-feeling, gives its Medical Officer of Health 20*l.* a year for the health-charge of 127,746, and such additional fees as he may pick up in inspecting ships under the Order of the Local Government Board as to cholera. Llandisfawr, rural, indulges in four Medical Officers of Health, for a population



of 16,000, and pays each "according to services." Louth, rural and urban, are typical districts. The former, with an acreage of 144,372 and a population of 24,260, gives its Medical Officer of Health a salary of 375*l.*; the latter, with an acreage of 2,560 and a population of 10,500, gives its Medical Officer of Health a salary of 125*l.* Both elect the same man, who devotes his whole time to his duty. Lyme Regis, urban, with a population of 2,400, pays a Medical Officer of Health half-a-guinea on every occasion it seeks his services. Maryport, with a population of 6,938, remunerates its Medical Officer of Health "according to circumstances." The arrangements as to the Medical Officer of Health in the following urban districts we give without comment. Maidstone, population 26,239, 100*l.*; Middlesborough, population 50,000, 125*l.*; Newport, population 30,000, 50*l.*; Worksop, population 10,000, 5*l.*; Oswaldtwistle, population 10,200, 10*l.*; Scarborough, population 24,200, 30*l.*

The foregoing facts have been culled pretty much at random from our list. They may be taken in part as illustrating some of the vagaries of local authorities who have repudiated Central interference with their Medical Officers of Health; and in part as showing how some local authorities have turned certain Central advice to good account.

*Combined Sanitary Districts.*—The sanitary districts of which we have information, that have been formed by a combination of several sanitary districts, are forty-six in number. These combined districts are best arranged in order of salary—indeed, hardly any other systematic arrangement is possible, unless an alphabetical one—as no definite relationship is to be discovered between the salary and the amount of work to be done for it, whether in relation to area of district or to population, or to both area and population.

It may be held that, in those districts which have been formed with the assistance of Inspectors of the Local Government Board, the chief thing aimed at was to get such grouping as would give the sum set down as a reasonable salary with the least cost to the several districts, irrespective of the duties for which the sum was to be paid. This list is so interesting and instructive that we give it entire.



## COMBINED SANITARY DISTRICTS.

Salary.	No. of Districts in Combination.	Area in Acres.	Population.	Name of District.
£80	2	88,000	12,700	Pickering.
£96	2	16,400	21,500	Golear and part of Huddersfield, R. <sup>1</sup>
£100	2	40,800	70,900	Aunstonley & part of Huddersfield, R.
£105	2	47,500	16,000	Shepton Mallet.
£120	3	73,800	15,600	Newquay, Cornwall.
£150	2	8,500	34,800	Bridgwater.
£200	4	33,500	25,500	Bingley.
£200	2	—	33,200	Worcester.
£225	3	60,100	21,700	Bournemouth.
£300	5	116,000	43,500	Chapel-en-le-Frith.
£300	2	116,000	23,000	Malton.
£300	2	181,500	23,700	Penrith.
£325	2	4,600	35,000	Reading.
£400	2	24,300	41,900	Atherton.
£400	4	169,500	38,700	Cockermouth.
£400	4	17,700	32,300	Cornholme.
£400	4	112,800	35,200	Dawlish, Devon.
£400				St. Mary Church and Torquay.
£400	3	303,600	47,500	Settle.
£450	5	157,700	57,800	Billesdon.
£500	2	59,000	30,600	Blackpool, U. <sup>1</sup> , Fylde, R.
£500	5	281,400	59,200	Bromyard.
£500	3	100,400	34,200	Goole and Selby.
£500	4	210,000	59,600	Mansfield.
£550	2	90,000	65,900	Chesterfield.
£600	5	175,700	56,100	Alcester, Warwick.
£600	2	40,800	79,900	Barkisland, U., Halifax, R.
£600	9	682,100	89,100	Bowness.
£600	6	176,000	95,000	Chertsey, R.
£600	6	130,800	74,300	Chester.
£600	3	50,600	55,300	Rawmarsh.
£600	2	69,000	35,500	Thornton.
£650	7			Grantham.
£700	5	240,900	87,700	Bishops Stortford.
£700	8	334,100	101,100	Dunmow.
£750	12	379,100	144,200	Abingdon.
£750	6	152,100	120,500	Barnet.
£760	8	447,200	135,600	Banbury.
£800	10	666,700	149,400	Atcham.
£800	14	400,000	131,700	Awre.
£800	3	241,600	69,300	Billericay.
£800	9	300,000	111,400	Blean.
£800	15	576,000	222,500	Brackley.
£800	7	200,000	96,000	Bromley.
£800	8	297,100	74,100	Caxton.
£800	7	236,000	86,000	Lillington.

*Sanitary Districts having several Medical Officers of Health.*—  
 These districts have been almost wholly formed according to  
 the advice first given by the Inspectors of the Local Govern-

<sup>1</sup> R. rural ; U. urban.

ment Board, that the Poor-law Medical Officers should be made the Medical Officers of Health. The number of districts of this kind already formed exceeds seventy. We cannot give the list entire, and we must content ourselves with taking samples from it, picking them out promiscuously from the alphabetical arrangement.

Ashby-de-la-Zouch, rural, has appointed six Medical Officers of Health for a total acreage of 51,952 and population of 31,532, and these officers it pays "according to services." Auckland, rural, with an acreage of 59,303 and a population of 69,153, has three Medical Officers of Health, to each of whom a salary of 50*l.* is given. Aylsham, rural, has seven Medical Officers of Health for an acreage of 68,123 and a population of 18,115, and to each Medical Officer a salary of 10*l.* is given as a species of retaining fee, and 1*l.* 1*s.* for every report of examination of premises. Barnstaple, with an acreage of 148,729 and a population of 37,406, has eleven Medical Officers of Health, who are paid 1*l.* 1*s.* for each report they make, and are allowed 1*s.* per mile one way for travelling expenses. Bath, rural, has an acreage of 27,719 and a population of 18,690, with five Medical Officers of Health, having salaries varying from 20*l.* to 25*l.* Belper, rural, appoints two Medical Officers of Health for an acreage of 65,764 and a population of 52,864, at a salary of 150*l.* each. Chippenham, rural, has seven Medical Officers of Health for an acreage of 58,100 and a population of 21,800, at salaries ranging from 9*l.* to 23*l.* Conway, rural, with an acreage of 30,778 and a population of 9,877, has two Medical Officers of Health, to each of whom a salary of 10*l.* is given. Croydon, rural, is arranged in three districts, each with its respective Medical Officer of Health, as follows: (*a*), area 7,590, population 9,423, salary 60*l.*; (*b*), area 6,269, population 3,110, salary 30*l.*; (*c*), area 10,205, population 2,466, salary 30*l.* Guildford, rural, with an acreage of 68,996 and a population of 24,461, has eight Medical Officers of Health, to each of whom it pays 10*l.* a year, and gives a fee of 10*s.* 6*d.* for every nuisance which he is called to inspect. Halstead, rural, has an acreage of 38,913 and a population of 18,453, with two Medical Officers of Health, each having a salary of 60*l.* Haverfordwest gives "fees according to circumstances" to four Medical Officers of Health, the acreage of

the district being 169,700, and the population 34,500. Kingsbridge, rural, has appointed five Medical Officers of Health to a district of 73,455 acres and a population of 19,700, and it pays them 1*l.* for every "case" they may be called to. Nuneaton, rural, is divided into two districts, each with its Medical Officer of Health: one district has an area of 4,200 and a population of 514, with a salary of 7*l.* 10*s.* attached; the other has an area of 1,500, with a population of 208, and a salary of 5*l.* Pontefract, rural, is also divided into two districts, each with its separate Medical Officer of Health, and each Medical Officer of Health having a salary of 50*l.*; but the one district has an area of 31,800 and a population of 10,000, the other district an area of 17,600 and a population of 9,400. Tavistock, rural, has appointed six Medical Officers of Health for an area of 159,317 acres and a population of 31,240, and to each of them it pays 25 per cent. on their salaries as Poor-law Medical Officers. Wantage, rural, for an acreage of 75,700 and a population of 17,360, has two Medical Officers of Health, to each of whom is given a salary of 25*l.* Warrington, rural, with an area of 29,500 acres and a population of 22,300, has also two Medical Officers of Health, to each of whom is given a salary of 100*l.* Wickham, rural, with an acreage of 73,000 and a population of 72,000, has two Medical Officers of Health, each with a salary of 50*l.*

There may be in the foregoing arrangements some harmony, hidden from crass unofficial judgment, between the appointments and the duties for which the appointments were presumed to be made. The appointment of the Poor-law Medical Officers as Medical Officers of Health was the scheme of the Royal Sanitary Commission, and although a crude one, it might in proper hands have been turned to good account. But here we have precisely the same kind of result as would have followed from putting a musical instrument for the first time into a child's hand, namely, a series of discords.

*Sanitary Districts, uncombined, which have elected a single Medical Officer of Health.*—Districts of this class far outnumber either of the other classes described. The list before us contains 340 of such districts. The appointment of Medical Officers of Health in the greater number of these districts has no doubt been made by the sanitary authorities independently of the Local

Government Board. Districts which had appointed Medical Officers of Health prior to the Public Health Act 1872 have been excluded from consideration. A great proportion of the districts included in this category are urban, and thus it would seem highly probable that the astonishing inconsistencies in the advice given to sanitary authorities by the Inspectors of the Local Government Board has had the effect of largely alienating urban districts from the Board. It is very difficult to deal with the districts contained in this category so as to convey any just impression of the characters of the appointments which have been made in them. We have analysed the appointments with reference first to area of districts, as perhaps most serviceable for use.

Twenty-two districts have areas of under 500 acres. In these districts the populations range from 1,300 to 19,000, the salaries of the Medical Officers of Health from 10*l.* to 50*l.*

Thirty districts have an area of 500 acres and under 1,000, the populations varying from 414 to 33,900, the salaries of the Medical Officers of Health from 10*l.* to 100*l.*

Forty-three districts have an area of 1,000 acres and under 2,000, the populations varying from 296 to 42,000, the salaries of the Medical Officers of Health from 3*l.* 3*s.* to 200*l.* The minimum salary is attached to the smallest population, but the salary attached to the largest (42,000) is only 50*l.*

Thirty-six districts have areas of 2,000 acres and under 3,000, with populations varying in number from 570 to 90,000. In these districts the salaries of the Medical Officers of Health range from 10*l.* to 500*l.* The Medical Officer of Health of the smallest population receives 10*l.*, of the largest 200*l.*

Twenty-five districts have an area of 3,000 acres and under 4,000, with populations varying from 2,600 to 127,700, the salaries of the Medical Officers of Health varying from 5*l.* to 150*l.* The salary attached to the district of smallest population is 10*l.*, to the district of largest population 20*l.* and fees for port sanitary work.

Twenty-three districts have an area of 4,000 acres and under 5,000. Their populations vary from 1,500 to 83,000, the salaries of the Medical Officers of Health from 40*l.* to 400*l.*

Forty-nine districts have an area of 5,000 acres and under



10,000, the populations varying from 3,400 to 355,000, the salaries of the Medical Officers of Health from 5*l.* to 500*l.*

Twenty-eight districts have an area of 10,000 acres and under 20,000, the populations varying from 2,500 to 112,000, the salaries of the Medical Officers of Health from 30*l.* to 160*l.*

Thirty districts have an area of 20,000 acres and under 40,000, with populations varying from 2,400 to 239,900, the salaries of the Medical Officers of Health varying from 1*l.* 1*s.* each inspection to 600*l.* yearly.

Of the remaining districts in our list (eighty-four), the areas vary from 40,000 acres to upwards of 100,000, the populations from 3,000 to 215,000, the salaries of the Medical Officers of Health from 20*l.* to 500*l.*

If we examine these appointments on the basis of salary, their general incongruousness is shown in another aspect. The following illustrations may be given :—

Salary.	Area of district in acres.	Population.
£10	Under 500 to 10,000	900 to 12,700
20	500 „ 10,000	1,600 „ 33,000
30	500 „ 20,000	3,000 „ 16,000
40	500 „ 40,000	3,700 „ 36,000
50	under 500 „ 80,000	6,000 „ 49,000
60	500 „ 80,000	5,200 „ 29,000
70	3,000 „ 100,000	6,900 „ 29,000
80	20,000 „ 60,000	9,000 „ 13,000
100	500 „ 80,000	5,600 „ 215,000
150	1,000 „ 80,000	9,000 „ 29,000
200	1,000 „ 100,000	11,000 „ 90,000
300	4,000 „ 15,000	42,000 „ 47,000
500	2,000 „ 30,000	84,000 „ 343,000

Mr. Stansfeld, in November 1872, defined his public health policy to be the development of local health self-government under the impartial guidance of his Board. Twelve months' application of his policy has been to bring about a state of local health self-anarchy, not because the policy was wrong, but because the means he adopted to give it effect were utterly bad.



# THE PRACTITIONER.

APRIL, 1874.

## Original Communications.

### STUDIES ON ETHER AND CHLOROFORM, FROM PROF. SCHIFF'S PHYSIOLOGICAL LABORATORY.

BY T. G. HAKE, M.D., F.C.S., M.R.C.P.

DURING a prolonged stay at Florence, I have enjoyed frequent opportunities of witnessing Professor Schiff's methods of experimenting on living animals in a state of complete anæsthesia, and have been so deeply interested in his researches, especially those on the comparative merits of ether and chloroform as anæsthetic agents, that with his sanction I propose to describe to you certain important results which I have myself witnessed. Much curiosity has been awakened here of late by some ill-advised parties having commenced proceedings in the law courts against the Laboratory of Physiology, but the process has been withdrawn, the accusation of cruelty to animals made against the eminent Professor having proved utterly futile and based only on ignorant rumours, as has since been admitted in writing by the principal complainant.

Whatever may have been the object of his researches, Professor Schiff during the period of twenty-five years has never lost sight of the laws of anæsthetic action, and his observations on this subject, as far as regards practical usefulness, rank among his most valuable results, those on the physiological

effects of ether and chloroform having a direct and most important bearing on human interests, while those on the curara poison invite further inquiry as to whether this agent is anything more than a paralyser of muscular movements. I may here add that he is at the present time earnestly occupied in seeking a substantial remedy for paralysis of the circulation occasioned by the use of chloroform, a fatality for which no reliable means of treatment has hitherto been found; galvanism, owing to its exciting very irregular contraction of the muscular fibres, which fails to exert any mechanical effect, having signally failed; and transfusion, which is more promising, being not at all times practicable. Further, that he has cause for hope of yet bringing this search to a successful issue.

That the heart is in a vital state favourable to its renewed action when paralysed by the influence of chloroform on the vaso-motor nerves, is demonstrated by Professor Schiff's practice of *artificial circulation*. He lays open the thoracic cavity, and, compressing the passive heart with his fingers, imitates in it the periodic movements of that organ. The circulation is restored, the nerves of the heart recover their force, and the organ finally resumes its spontaneous action. Before long, signs of returning sensibility become apparent, the reflex action of the eyelid returns, and the animal is now capable of being brought back to cerebral consciousness; it would, however, be both cruel and useless to extend the experiment so far.

In all his experiments the preliminary condition with Professor Schiff is that the animal employed should be in a complete state of insensibility, a rule which he never deviates from, it being a fixed principle with him never to inflict suffering. Consequently, doubts having arisen in his mind as to whether the curara poison acts as an anæsthetic, or only paralyses movement, and so prevents the outward expression of pain, an anxious question has been opened to him, this agent being much used in physiological experiments on living animals under the notion that it destroys sensation. In the new edition of his work, "*Sopra il metodo seguito negli esperimenti sugli animali viventi*," expressing these doubts, he says: "We have at the present time made fresh experiments on frogs, and have found that curara given in large doses really destroys sensibility: but

it does not act as an anæsthetic until a considerable time after having destroyed all voluntary movements and automatic respiration. In the mammalia it is impossible to make such experiments as indicate with precision how soon sensation ceases after the commencement of poisoning. But it is certain that in these also sensibility still exists after all voluntary movements have ceased, and we have no right to consider that a mammal poisoned with curara is insensible because it is unable to manifest its sensibility by means of ordinary movements and cries. Under these circumstances we are bound to admit the possibility of consciousness and pain persisting during the whole period after poisoning, while an animal responds still to any strong impression with manifest contraction in the vascular or in other unstriated and involuntary muscles—contractions which we are not able to recognise in a direct manner, but which are manifested by the complicated means afforded us by science. It is possible, I admit, that sensation may have already ceased, but one cannot prove it, the proof being wanting which other anæsthetics afford us in man. As a friend observed to me, ‘In experiments for measuring the pressure of the blood, made with the exclusive use of curara, and in which the pressure was found at the *normal height*, the curara has acted only as a tranquillising agent which in impeding movement conceals the pain from the bystanders. It is no other than hypocrisy wishing to convince oneself and others that the animal under curara never feels pain.’”

While curara acts as a paralyser of all movements, voluntary, reflex, and irritative, the temptation of using it as a supposed anæsthetic is very great, its effects leaving intact the influence of the irritation of a sensitive nerve on the automatic movements of the circulation and respiration, to those who would make this an object of study. Professor Schiff only contends that until more than theoretical evidence of its anæsthetic properties can be adduced, the use of curara is unjustifiable; and he urges this view the more strongly on the ground that we have no right to inflict pain for the purposes of research.

Professor Schiff's observations on ether and chloroform have a direct bearing on surgical practice, though not pursued with that view, but solely for the advancement of physiological science

by means of the most perfect methods. "We adopt ether and not chloroform," he says, in the work already cited, "because a very extensive experience has shown that etherisation pushed to the very last stage of insensibility is never dangerous to life so long as one maintains the act of respiration. And even if one presses the inhalation of ether yet further, so that the respiratory movements cease, or, in other words, the appearance of death is complete, life is never menaced, if only at the moment of the paralysis of the thoracic walls inhalation is interrupted and a species of artificial respiration is immediately commenced by means of periodic compression of the thoracic parietes themselves.

"Chloroform has been preferred to ether because it acts more quickly, and its use is more agreeable to the patient, who dislikes the odour of ether. But chloroform has a paralysing action much greater than that of ether, and in like manner, at least in man and the mammalia generally, has a special influence on the nerves of the heart and of the vessels. If chloroform is pushed so as to produce a considerable weakening of the respiratory movements, the interruption of the inhalation may, in a majority of cases, lead to the re-establishment of respiration and afterwards of sensation; but sometimes, a short time after the commencement of inhalation, the force of the circulation is so enfeebled that it no longer renews, fast enough, the blood in the lungs. The blood of the body no longer comes into necessary contact with the atmospheric air introduced by respiration into the lungs.

"Death is sometimes sudden, but it may be preceded more or less by signs of sinking of the pressure of the blood in the vessels. The cases in which paralysis of the circulation shows itself while respiration continues are comparatively rare, but the annals of human surgery record many examples, and we have ourselves observed some in animals. If the action of chloroform is prolonged until respiration ceases, we are not even sure of being able to revive the individual after having re-established the respiratory movements, for these often again cease owing to the disturbance of the circulation, while these same movements, if restored after the inhalation of ether, become *always* more frequent in the individual when left to himself.

"We are able to say that in the present state of science the medical man *is responsible for every case of death occasioned by the application of ether*, because a careful watching of the respiration is capable of preventing death, whilst the lethal effect of chloroform depends in part on individual predisposition which the physician is unable to recognise."

To explain this latter statement, it should be understood that both ether and chloroform, pushed to the last stage of their action, give rise to paralysis of the respiration, vessels, heart and motor nerves, but that ether invariably produces its effects in the order of sequence now given (life of course being sustained in all cases by artificial respiration when automatic breathing has ceased); while chloroform sometimes produces paralysis of the vessels in the first instance, then of respiration, and finally of the heart. The result of the action of chloroform is thus variable; it frequently happens that its effects manifest themselves in the same order of sequence as those of ether, only much more rapidly; and it also happens that they follow each other in an inverse order as respects the first two phenomena, paralysis of the respiration and of the vessels. It is this variable action of chloroform which the physician is unable to foresee and to provide against in individual cases, and to which the danger to life is traceable. Very often at the beginning of the inhalation of chloroform by the trachea the vessels become at once paralysed, the pulse is insensible, and death follows rapidly with a deep inspiration. All this Professor Schiff has frequently verified by experiments on dogs and rabbits, in which during inhalation the manometer has been constantly in connection with the carotid artery.

That the heart is the last of these three factors of life which dies, the Professor clearly demonstrates in the following way. When by the action of chloroform the pressure has gone down nearly to zero, and there is no pulsation visible, compression of the thoracic aorta between the crura of the diaphragm, or the mere compression of the abdomen, restores to the pulse its strength and frequency, and causes the pressure in the manometer to rise to a considerable height, very often to 100 or 120 millimeters. This fact proves that the heart, notwithstanding its apparent paralysis, is still able to maintain the almost normal



pressure of the blood, and to contract with more strength than before when the pressure is acting upon its walls and opposing itself to their contraction. The apparent paralysis must depend on this cause: the vaso-motor nerves are paralysed, inasmuch as all the vessels of the body are dilated; their increased capacity retains the blood, now no longer returned to the heart, which becomes bloodless in a secondary manner. Compression of the abdomen, or ligatures to the upper and lower extremities, produces a compensation for the increased capacity of the blood-vessels, and in this way the circulation becomes more normal, while before it had suffered, not through paralysis of the heart, but from paralytic dilatation of the peripheral vessels.

As regards the comparative value of ether and chloroform, Professor Schiff continues:—"Our own experiments bearing on this argument enable us to say that in more than three thousand cases we have adopted etherisation with a view to preserve the life of animals, and that with the few exceptions indicated elsewhere (Memoir on the Laryngeal Nerve), not a single case of death occurred. On the other hand, chloroform has cost us a considerable number of animals when I have wished to push anæsthesia to its ultimate stage.

"Our experiments confirm more and more that in etherisation the pressure of the vessels maintains itself to a height almost normal and always compatible with the continuance of life even after the cessation of automatic and the substitution of artificial respiration, so that the mere continuance of breathing gives us a safe warranty of the vitality of the individual. Often in experiments made with this view, we have seen that at the moment of the cessation of automatic breathing the circulation was still in so normal a state that the commencement of asphyxia indicated still the *asphyric height* of vascular pressure as measured by the manometer; that is, instead of falling before death, the pressure rose through the accumulation of carbonic acid, which, as is known, is an irritant of the vascular system and of its nerves.

"When, after the cessation of breathing, one at once applies artificial respiration with air that is passed over a stratum of ether, so that etherisation is still kept up, one is able, by regulating the quantity of ether that is mixed with the air, to continue for hours the etherisation of the animal, which no longer breathes

spontaneously, without the pressure of the blood being notably diminished, and without danger to the life of the animal, which one can always resuscitate by introducing pure air into the lungs.

"It is true, however, that the pressure of the blood always diminishes slightly, so that after two hours it may have fallen, for example, from 120 to 80 millimeters; but such a fall is not prejudicial to life. One is able in these experiments to regulate with facility the quantity of ether, commencing with a low temperature of the vessel that contains it. If one finds that the animal shows a ready tendency to recommence certain automatic respiratory movements, Wolf's bottle, which contains the ether, and is connected with the bellows and manometer by means of india-rubber tubes, is brought rather nearer to the body of the animal so as to increase the heating effect.

"It is otherwise with chloroform. In animals in which, under the influence of this agent, the pressure of the blood has been examined with the manometer, one finds that the pressure is already considerably lowered before automatic respiration has ceased; and we have frequently seen the pulse disappear almost entirely in the manometer, whilst the pressure fell to 25 or 30 millimeters, and the dog still breathed spontaneously.

"In dogs in which one employs artificial respiration from the commencement, causing the air to pass through a bottle containing chloroform, so that in entering the lungs it is but feebly loaded with this agent, we have seen the pressure, sometimes immediately, sometimes after a longer period, lower itself almost to zero, while the extremely weak pulse which the manometer recorded has also ceased soon afterwards, the respiration being continued as at the beginning of the experiment. It is therefore certain that in these cases it was not the cessation of the respiration, nor its weakening, that killed the animal. This becomes yet more evident through the experiments in which, after the cessation of vascular pressure, when the pressure was that of blood almost at rest, one suddenly replaced the respiration of chloroformed air by normal air without being able to save the animal.

"After a few forced inspirations one sometimes sees the automatic breathing of the animal recommence. This may be up to

two, four, and, as in one case, ten automatic inspirations ; but the pressure of the blood does not rise, and the dog dies through cessation of circulation.

“ Fortunately things do not always go thus. It is possible to find that a very limited quantity of chloroform mixed with the air may maintain a state of apparent death without any automatic respiration, and with an extreme fall of arterial pressure that yet admits, after half an hour's continuance of chloroformisation, of the re-establishment of life on pure air being introduced. But the keeping the animal alive in such an experiment is always uncertain : we cannot make sure, as with ether, that the animal will be revived. We are never able to say, and this is a great point, what the limit of chloroform is in the inspired air, which affords us a certainty of the animal being restored.”

These results of Professor Schiff's labours, which I have frequently seen demonstrated by him, have so impressed me with their importance that I would express a hope that the questions they involve may undergo a fresh discussion. I have translated his own words with a view to give fuller weight to his opinions. He considers that chloroform should be banished from practice as an anæsthetic agent, except in cases in which extraordinary resistance to the effect of ether shows itself, in which instances it might be allowed to mix a little chloroform with it in order to produce the commencement of anæsthesia, which should afterwards be continued with pure ether.

As regards the precautions during the use of ether which Professor Schiff points to in referring to his memoir on the laryngeal nerve (“ Ueber die angebliche Hemmungsfunction des Nervus Laryngeus Inferior,” published in *Moleschott's Untersuchungen*, 1867), they are too important and interesting to be passed over. The paper containing them was written a short time after the publication of the discovery of Rosenthal that irritation of the superior laryngeal nerve has an inhibitory influence on the respiratory movements. After having shown that in rabbits, guinea-pigs, frogs and toads, many sensitive nerves of the anterior part of the body, and in some cases all the sensitive nerves of the skin, have an inhibitory influence on the respiration and on the heart's action when they are slightly irritated in a continuous manner (by mechanical means),

and that in this respect the superior laryngeal nerve has not an exceptional position, but shows only a quantitative difference, the author mentions that in normal dogs and cats he did not find the same effect from irritation of sensitive nerves; but that during the return of these animals from a very deep etherisation or chloroformisation, while the respiratory impulse is weak but regular, he did not find it difficult to show this inhibitory influence in many sensitive nerves of the anterior part of the body and over the middle line of the back along the course of the vertebral canal, on the pulse and respiration.

The animal is etherised or chloroformed until respiratory movement has quite ceased; then artificial respiration, by interrupted compression of the abdomen and chest, is employed. Under ether the recovery is certain; and when the animal begins to breathe automatically, but still weakly, there is a moment in which any strong mechanical irritation of the nerves above indicated produces *a new and a more persistent asphyxia*; the diaphragm is relaxed, and it seldom happens that the animal is recalled to life by a return to artificial respiration. The persistence of this new species of asphyxia is now explained by Professor Schiff's more recent researches on the subject, in which the mechanical irritation of a posterior spinal root, previously laid bare, produced paralysis of the circulation. This is a reflex paralysis of the same kind as that which Ludwig and Cyon produced in a slighter degree in normal rabbits by the irritation of the *nervus cardiacus longus*.

This experiment can be made only during a short period on the return of the animal from the highest degree of etherisation or chloroformisation; and when this very brief period is passed, irritation of the nerve has no longer the influence now pointed out on the respiration, but acts in an inverse manner on the pulse, increasing its strength and at the same time the pressure of the blood. In connection with this phenomenon Professor Schiff observes that, considering the analogous manner in which sensibility is reflected in the spinal marrow of these animals and of man, this experiment affords a warning of much importance to surgeons, not to continue an operation immediately on a patient's recovery from the excessive action of anæsthetics, but to wait until respiration has been energetically restored, other-

wise a new and generally fatal asphyxia may be produced. In his last and not yet published experiments on this subject, Professor Schiff killed some dogs in the first period of return from asphyxia produced by ether only, by raising with a tentaculum one or two posterior roots of the dorsal nerves; while in other dogs prepared in the same way, in which the same operation preceded *the division of these nerves*, in a somewhat more advanced period of the return of respiration, that function continued in its regular progress up to the time of consciousness being restored.

Before concluding, it may be of interest to many to learn that the Laboratory of Physiology in Florence, of which Professor Schiff is the illustrious head, is a public establishment on a very large scale, where every kind of apparatus by means of which physiological science may be advanced is collected; while the courtesy of the Professor holds out every inducement to scientific men to visit the institution, as well as to witness and join in the investigations which are being constantly made within its walls.

FLORENCE, Feb. 25, 1874.



## SPASMODIC ASTHMA.

BY J. H. WEBSTER, M.D.,

*Physician to the Northampton General Infirmary.*

THE accompanying case of spasmodic asthma due to pneumogastric irritation has the causes and symptoms of that distressing though interesting malady so well marked, that a detailed account with remarks thereon may be worthy of being recorded in the pages of the *Practitioner*.

S. W., aged 23, in service as cook, dark hair, fair rosy complexion, moderately well nourished, native of a village near Northampton, was in the enjoyment of full and robust health until twenty years of age. She never had any previous pulmonary ailment, her only illness having been an acute attack of rheumatism in early life.

At that age she went into service as cook in a low-ceiled kitchen with stone floor, where all the cooking was performed by gas-stoves. The room was hot and close, and the smell from the burning gas often very offensive.

A few weeks afterwards she was seized in the middle of the night, during a first sound sleep, with tightness and oppression of the chest, suffocative shortness and quickness of breathing, inability to take a full inspiration, with a wheezing and whistling respiration. This would last some two hours, when cough would supervene, with expectoration of some phlegm gradually increasing in quantity for the ensuing four or five hours, when with the increase of expectoration the spasm would subside, the tightness of the chest relax, and the breathing become slower, fuller, easier, and more tranquil. The attack would last with abating severity until about three o'clock in the afternoon, or a

general period of twelve hours from its commencement at 3 A.M., when she would feel herself quite well again.

These attacks recurred every third week, occasionally only keeping off until the expiration of the month, appearing each time of the catamenial period. It should be mentioned here that later on, when the fits became more frequent, they were always attended by a menorrhagic loss. She admits that she was very neglectful of the state of her bowels, being usually very constipated, and that she subsequently learnt to anticipate an attack from noticing that the urine became for some days previously very turbid with a heavy red deposit.

Attributing her illness to living in the impure atmosphere of this gas-cooking kitchen, she changed her situation to another in Northampton, where the kitchen was larger, airier and above ground, and with open fire cooking. Here the attacks were less frequent, coming on only at the monthly periods, always at 3 A.M., and lasting until the middle of the following day. Owing to the regularity of their recurrence and her consequent inability to do her work, she had after twelve months to leave that place, when she returned home to a bleak exposed village. Here she fared worse, the seizures being more frequent and lasting longer.

About this time, namely the latter end of 1872, she discovered that eating pork any how cooked, salted beef, ham, pastry, cheese, frizzled bacon, taking ale or porter at supper-time, or any exposure to night air, generally brought on an attack at 3 A.M. the following morning; but there was this difference noticed in the seizures induced under those circumstances, namely, that they were unattended by any uterine flux.

She had then been advised to limit her beverage to weak brandy and water, and upon the accession of every attack to take two tea-spoonfuls of ipecacuan wine, to induce vomiting, which was always followed by speedy marked relief.

In January 1873 she engaged herself as cook with a family at Silsoe, in Bedfordshire, a low-lying, damp locality. Here the attacks recurred at the same hour every Sunday morning for a period of ten weeks, and were always attended with uterine hæmorrhage. She assigned as a probable reason, the extra amount of domestic work on the Saturday.

She being apparently a very confidential servant and no mean culinary *artiste*, the family on visiting Cheltenham took her with them. There, for the first four of a seven months' residence, she had an immunity from any attack; during the latter three months they returned, and during the last fortnight she had two more violent attacks, lasting two days on both occasions, attended by menorrhagia of three days' duration.

The urine, as I have before mentioned, was always heavily loaded before the fits; subsequently it became limpid and very much in excess of the usual amount of secretion.

Of her own accord she mentioned that she had been much vexed with herself that previous to these attacks she had felt so mentally excited, touchy, irritable in temper, and even quarrelsome with her many fellow-servants, it being a large establishment.

Saving the cough and expectoration on the subsiding of a fit, she was during the interval entirely free from either; though conscious of an amount of wheezing at the chest for a length of time afterwards, but not aware of any such noisy respiration previous to an attack coming on, which she described as "sudden and during sound heavy sleep." She returned home from Cheltenham last August, and finding herself no better, but rather worse (even though every care as to diet and exposure was observed), at her parents' home, took lodgings in Northampton in the beginning of November, when she came under my notice.

As I was not then in possession of the above history, her weekly attacks of asthma were considered as owing partly to constipated bowels, deficient renal secretion, to hysteria, and possibly sleeping in a too-crowded room. She was prescribed an aperient pill with a stimulant alkaline antispasmodic, with instructions to have better ventilation in the sleeping room, and, an attack coming on, to produce a vomit by ipecacuan wine, after drinking freely of warm barley water.

As she did not manage herself to my satisfaction, I admitted her to the infirmary, thereby affording myself more favourable opportunities of judging of the predisposing and exciting causes of the malady.

There was no lesion of the cardiac action, it being equable

and normal; slight bronchitic râles over the upper lobes of both lungs and middle lobe of right; the bowels much constipated; the urine scanty, thick, and depositing lithates in abundance.

The treatment was directed to daily gentle opening of the bowels, increasing and neutralising the excessively acid secretion of the kidneys, with counter-irritation of the chest by an iodine solution.

Before the alvine and renal functions could be brought into activity she had an attack at the usual hour in the morning, attended by a profuse hæmorrhagic discharge. This was so much in excess as to call for some suppression, which was accomplished by a few doses of cannabis indica; an anti-menorrhagic remedy, first brought under the notice of the profession by, and of which I hope hereafter to say something in, the *Practitioner*.

There was only one other severe attack whilst in the house, due I believe to a little over-indulgence in roast beef and plum-pudding last Christmas Day.

There were several threatenings of attack, which by the administration of a few minims of chloroform at first, and lastly by a pill of one grain of the extract of belladonna, were averted.

She left the house after seven weeks' residence, expressing herself better than she had been for the past three years.

There are many points in the pathology of this case deserving of consideration, especially in connection with that mysterious complaint, hay asthma.

Admitting the moot point of the muscular irritability and contractility of the bronchi and smaller ramifications of air-tubes, I would confine myself entirely to the probable predisposing and exciting causes of such spasmodic action.

In the first place, we may justly assume that the continued breathing of heated air, charged with much mephitic moisture from gas-burning in a close room, was a source of irritation of the pulmonary plexus of the vagus.

Secondly, that the depuration of the blood from carbonic acid must have been much interfered with by the quantity existing in the respired air.

Thirdly, we may be justified in assuming that lengthened

irritation of the pulmonary plexus would be reflected on to the gastric plexus.

Fourthly, deficient function, alvine and renal, would induce, in excess neurosis of the great sympathetic.

Fifthly, that imperfect oxygenation of the blood may account for the excessive acidity and loaded condition of the urine; as also an amount of hepatic congestion evidenced by the attendant uterine flux under the irritation of habitually constipated bowels.

Sixthly, it should be carefully borne in mind that whenever the main *lines* of the nervous organisation of animal life, all connected together by innumerable interlacing filaments, have from one cause or other been subjected to frequently recurring excitation, they acquire an irritability which from any slight cause impressed upon any one point or link in the chain, whether central or peripheral, may throw the whole group into disturbed action.

In reference to this last, as merely explanatory of the case in hand, I would suggest for consideration those wonderful and unintelligible symptoms evidenced in periodic epilepsy, which are seldom if ever fully developed in a first attack (there being initial stages) of the malady, the causes of which I am sure and certain in very many instances the discerning physician can check and remove if on their outset they come under treatment, but, neglected, "*vires acquirit eundo*," become confirmed, and then too often baffle the highest remedial treatment.

During the past few years a theory of sleep has been propounded, as due to slight venous congestion and to the sedative influence upon the cerebral organs from an excess of carbon in the circulation. If this may be assumed, it may also be inferred that, in cases where, from continued breathing of a moist, hot, impure air, the arterial fluid has too much the character of the venous from deficient decarbonisation, and where not only sleep would be much heavier and sounder, but the lungs would be labouring in their function of adequately excreting effete matter from the system, there would, in consequence, be an active predisposing cause of irritation of the pulmonary branches of the vagus. Again, if this condition persists in the female economy on the approach of the menstrual period, when some



not well-understood relief of the whole system, far beyond that from the mere escape of a Graafian vesicle, *is called for*, the generally accompanying neurotic excitement, which in our ignorance we term "hysteric," may justly be presumed to have an influence through the great sympathetic upon the pulmonary functions, thereby augmenting existing irritation or causes of irritation.

The frequent occurrences of vicarious menstruation, whether pulmonary, gastric, or nasal, will support this view.

Through the gastric plexus, which, as I have before mentioned, will ultimately, sooner or later, sympathise with the pulmonary (and here I would add *vice versâ*), a hyperæsthetic condition of the gastric mucous membrane may under the circumstances be engendered, which might cause articles of diet and drink, previously taken with impunity, to be painful, irritating, and indigestible. Consequently there would be mal-assimilation, flatulency distressing the heart's action and pulmonary circulation; acidity and imperfect chyme, affecting renal, hepatic, intestinal, and perhaps cutaneous functions, inducing constipation alternating with diarrhœa; functional derangements of the whole alimentary canal, disturbances of animal life, telling their tale, through the medium of the sympathetic, at head-quarters of organic life.

Lastly, the enfeebling influence upon the system of frequent and excessive menorrhagic losses, induced by a mechanical uterine irritation from a constantly overloaded sigmoid flexure, should be taken into account, as few in the present day will not acquiesce in the Abernethian dogma of the convertibility of the terms debility, irritability.

To such a group of predisposing influences would I attribute the reason why slight exciting causes, otherwise inadequate, were attended with such violent results.

They always came on in the middle of the night, awakening the patient from sound lethargic sleep; there never was any attack during the daytime.

Defective functional excretion embarrasses the organs of the system; the sensibility and irritability of those organs become cumulative up to a certain point, when spasm of the bronchial circular muscles sets in, most probably with the "dry asthmatic

symptoms;" subsequently congestion of the mucous membrane, followed by effusion, the "humid asthma," when, after copious expectoration, relief to the system is obtained and the attack subsides.

Any theory of the causation of a disease is very fairly strengthened or invalidated by reasonings drawn from the "Adjuvantia" and "Sedentia."

Upon the latter sufficient may be inferred from the above description of this case. As to the former, the continued respiration of impure air, the dyspeptic symptoms, the constipation and overloaded renal secretion, and oft-recurring menorrhagia, with the times and circumstances under which the attacks would supervene, pointed directly to a series of probable causes of the malady, which being obviated, relief might be expected. Such unquestionably was the result; and though stimulating into greater activity the emunctories of the system did not at first remove the tendency to a periodic recurrence, yet its continuance, and the occasional exhibition of a nervine sedative, as a few minims of chloroform or grains of belladonna, sufficed to allay the habitual sensibility of the pneumogastric and stop any threatened invasion.

Our most able writers upon asthma hardly, I think, attach sufficient importance to the excreting function of the lungs. Nature will invariably make that function supplementary to deficient action of other excreting organs, or it of itself will take on proportionate action when all others are labouring to eliminate some *materies morbi* from the system; to wit, in the exanthemata, gout, uræmia, and others. So that I question the advisability of checking in the first instance, by sedatives only, excessive bronchial secretions, but rather attempt removing that necessity by stimulating other organs into more vigorous action.

In ordinary cases of bronchitic asthma, I have seen but little benefit resulting from expectorants and sedatives given until the digestive, hepatic, urinary, and intestinal functions have first been regulated and kept up for some time in gentle but continuous action. When such has been established I have seen the happiest results from a grain of blue pill with four of the pil. scillæ comp. night and morning, with four-minim doses of chloroform suspended by mucilage in camphor mixture taken

upon any impending attack: and the *modus operandi* of such remedies is quite intelligible.

The atmospheric and constitutional conditions under which catarrhus æstivus, or hay asthma, is induced are at present obscure and unsatisfactory. Alleged exciting causes, whether ipecacuan, pollen of the *Anthoxanthrum*, chlorine vapour, or other irritating substance, seem inadequate to account for such sudden and violent symptoms. Should, however, the above reasonings be tenable, of a cumulating influence of predisposing causes, it may be somewhat more intelligible why some trifling exciting cause, such as the above-mentioned, may so suddenly develop such grave attacks. It is curious that genuine hay asthma is a disease of early summer, when warmer and consequently more humid atmosphere renders the pulmonary evolution of carbonic acid less free than in colder and drier weather; also that females are more prone to such attacks just before the catamenial periods, and especially those who are plagued with emotional irritability of temperament.

Leaving out of the question asthmas complicated with cardiac, tubercular, pleuritic, or emphysematous lesions, I fully entertain an opinion that those arising from functional causes can be most surely and permanently relieved by continued attention to the cutaneous, hepatic, urinary, and intestinal emunctories, by which we reduce to a minimum the excretory function of the bronchial mucous membrane.

# OBSERVATIONS ON THE USE AND EFFECTS PRODUCED BY THE EMPLOYMENT OF COLD IN TYPHUS AND ITS VARIETIES.

BY DR. ALIVEY.<sup>1</sup>

TAKING a man from his bed in what is called a burning fever, placing him in a tub, and throwing a pail of cold water on him, at any time of the year, but more particularly during the severity of winter, appears to the ignorant a practice so extremely harsh, cruel, and unnatural, that where the practice is not common, or the parties rational and well informed, if success do not follow the means made use of, the practice, especially in that particular case, is sure to be called in question. In order therefore, under such circumstances, that we may not be altogether losers of the good effects to be produced by the employment of cold in fever, I thought my time would be well employed by informing the Society of the method I have lately made use of in several instances to remove the prejudice of the patient and friends, and at the same time acquire some of the advantages procured in fever by the employment of cold affusions. But in order that the observations I have to make respecting the relative powers of heat and cold upon the *sensations of the human body* may be better understood, I shall take the liberty of first bringing forward a few *postulata* upon the subject of cold.

<sup>1</sup> The date of this paper is 1801, and we have thought it might be interesting to reproduce a record of the impression which Currie's cold-water practice had made before it unhappily sank into neglect and oblivion. We owe the MS. to the kindness of Dr. Wilson Fox, who found it in an old manuscript volume of "Reports of the Proceedings of a Medical Society in the Country" which has come into his hands.—ED. *Pract.*

I set out with considering cold as only a diminution of heat. Heat in all its degrees within a certain range proves stimulant to all animals, in an exact proportion of its degree.

The degree of cold which produces death in some animals is still stimulant, since it can sustain the phenomena of life in others. There is a certain place in the scale of heat called an agreeable state, and the various points below this are called cold, the effects of which are to diminish the living, moving, and acting state of all animals.

But this degree of agreeable temperature does not prove such to all bodies, since the excitability or capability of being acted upon varies in different persons and animals. The effects of excessive cold are a disagreeable sensation, a desire of warmth, universal tremor, numbness, and torpor. When in this situation, if moderate warmth be applied, a sensation analogous to that called burning is induced; but when the cold is carried further, either in duration or degree, and in no degree diminished by the intervention of heat, its effects become similar to burning from heat, and both (*i.e.* heat and cold) similar in their painful and corroding effects to that of the fossil acids or caustic alkalies. When cold is carried further, first all pain, then all uneasy sensations, and lastly all feeling, ceases; a universal suspension to both voluntary and involuntary motion, together with the action of all the functions. Deep sleep of course follows, which, if allowed to continue for no great length of time, is not to be overcome by any exciting power, but terminates in death. Cold applied in a more moderate degree, but continued longer, will produce death as in the former case, although the intermediate parts of the process may be in some degree different.

The redness of the face and vigour of the body so common in cold countries and cold seasons do not arise from the application of cold itself, but from the brisk exercise to which the person is impelled by the inconvenient sensation of cold, the addition of clothes, the use of stimulants in much larger quantities, the employment of strong drink, and the stimulus of food.

Hence it appears that cold does not produce this vigorous state, but the stimuli, and on such occasions evidently counter-acting the effects of cold. If the effects of cold be separated



from these counteracting powers, this vigour will not appear: hence a person *in the cold bath* will not experience these exhilarating and invigorating effects.

Death is produced by cold diminishing the sum total of the stimuli necessary to support the phenomena of life. In small-pox, where debilitating medicines are enjoined, cold is used as one, in which case it procures a free perspiration and proportional diminution of eruption. The same remedy is equally productive of good consequences in the measles, phrenites, and other diseases of high excitement.

Cold has been said to produce acute diseases, such as catarrh, acute rheumatism, pneumonia, &c., and therefore could not be a debilitating power; but these diseases arise from a too sudden application of stimuli after an exposure to cold; hence, when a person who has been thus exposed returns home after being for some time under the influence of cold, is seated near a warm fire, warm and stimulating food, spirituous liquor, &c., being administered, and lastly a warm bed with additional covering, this person will perhaps sleep very comfortably; but when he awakes it often happens that some one of these diseases is seen in its incipient stage, produced from the stimuli applied, not from the cold, but rather from the heat.

Since therefore, after the application of cold below what is called the agreeable point, shrivelling of the skin, a dislike to motion, symptoms of numbness and torpor, a failure of intellectual operations, drowsiness and sleep are produced, may we not conclude that cold below this point proves debilitating to all animals whatever, and exactly in proportion as is the diminution of heat?

These *postulata* being granted, I shall proceed to observe that previous to the Brunonian system being promulgated by its author, cold has been used in the treatment of fever in Italy and other warm climates; but as at that time practitioners had no regular data on which to found their practice, they were not generally successful.

Since the action of cold upon the human body has been more satisfactorily explained by those illustrious men Brown and Currie, the practice of giving cold drink and using cold ablutions at certain times and periods of fever became

more commonly employed, and has assumed a more decided systematic form.

Every practitioner, particularly in country practice, is well acquainted with the influence and effects produced in minds prejudiced against what is considered experiment and innovation; and hence, although he may have read the work of a Currie or a Brown with the greatest attention, and have the strongest reason, in cases of fever, &c., for recommending and endeavouring to enforce the trial of cold affusions, yet, as he cannot take upon himself to promise that it will certainly effect a cure, and the practice in itself appears terrible, many are the objections which will frequently be raised to oppose it; and if after making trial of the means he do not succeed, he will in future become more timid and will be inclined to wait until general practice remove prejudice, and the patient and friends attend to the advice with pleasure.

To obviate the inconvenience I have had recourse to a method in most instances equally beneficial, and certainly much less to be dreaded. As I presume Dr. Currie's book is in everybody's hand, as well as I should hope the *Elementa Medicinæ*, I shall not here attempt to abridge any part of it, but taking it for granted that the time when to employ the application of cold is well understood, if the patient or friends object to the affusion I have in several instances reaped great advantage from allowing the patient, during what I would call the acme of pyrexia, cold drink *ad libitum*, at the same time making use of tepid spongings with water heated to the temperature of 92° of Fahrenheit's scale, in which I have added ℥ij of the following mixture to every pint of warm water:—

℞ Aceti communis ℥iij

Natron muriatum ℥vj ℥

But it is to be particularly and constantly held in view that when this practice is adopted the heat of the surface *must be* steadily above what is natural, not the smallest *sensation of chilliness or tendency to perspiration* being present. That species of fever termed typhus gravior has occasionally shown itself with us this spring and summer, and generally has been accompanied with petechia; but by far the most prevailing disease has been and is the typhus mitior, which commonly extends to

the twenty-first day and sometimes to the twenty-eighth or even longer; at any period of which, previous to the fourteenth, the depression of strength not being very great, but the heat accompanying the exacerbation being *very considerable*, I have found great benefit in pursuing this practice; and in some particular cases, attended with the most alarming and threatening symptoms, cold drink and tepid spongings had the effect in the opinion of everyone present of shortening the duration of the exacerbation and curtailing the period of the fever.

This was remarkably evinced in the case of Mr. Ferrier, surgeon, of Kimbolton, who on Thursday, March 12th, 1801, being exposed to some of the causes producing fever, had a regular attack come on in the night, and continued getting worse until the eighth day, when his friends sent for me to visit him in consultation with Mr. Peak and Mr. Mackie. During the preceding evening the paroxysm of fever was very considerable, accompanied with excruciating pain of the head; and during the remission, though no complete apyrexia was ever present, he mostly continued in a state of torpor, dulness, or insensibility, and we had every reason, from observation and analogy, to suppose that the fever would continue to the end of the third week, or wear our patient out before.

From the time of my being sent for the cuticular heat had greatly diminished—I should presume from the effects of considerable perspirations; and during the height of the exacerbation I never could understand that the heat was so great as frequently shows itself. On this account, as the disease had advanced towards the ninth day, I thought it not advisable to recommend either the cold affusions or tepid sponging, and the more so as my patient was not in a state to acquiesce: still, as the exacerbations of the fever were very severe, and, when at the height, the heat and thirst and restlessness excessively troublesome, I allowed him, during the time the extreme heat was the greatest, cold water for drink in any quantity agreeable to him; and such was the refreshment and benefit he experienced from its use, that although very delirious at the time, yet he became attentive to the noise of the pump under his window, and apparently listened to it with uncommon pleasure. By persevering in this practice, and employing with it the means

generally made use of in these cases, this fever very unexpectedly terminated on the fifteenth day, and our patient very soon afterwards recovered.

I have had two cases since then in almost every respect similar, in which the same practice accompanied with tepid ablutions succeeded equally well.

Here I would call for the most serious attention of the Society in requesting them to be particular in recollecting that there is a species of fever in many respects similar to the synochus of Cullen, in which the whole thoracic viscera appears from the commencement more than usually unmoved, and catarrhal symptoms are frequently very troublesome. The senses of hearing, and taste, and feeling, are throughout the disease uniformly acute, and the heat of the surface frequently up to  $108^{\circ}$  or more of Fahrenheit's scale, with great restlessness, delirium, and almost incessant talking. Although the heat of the body is here so great, yet the patient invariably shrinks from and is hurt by the impression of cold. In fevers of this description, the practice of employing cold, in any form so as to prove inconvenient to the patient, has been found baneful and ought to be abstained from altogether. As this particular kind of fever differs from most, so it can hardly be confounded with any other, if the practitioner will take time to form a just diagnosis. Fortunately for mankind, this variety in fever is not very frequently met with, for although it is said to be never epidemic nor contagious, and its first attack mild when compared with many other forms of the same disease, yet in almost all the cases which Dr. Currie has met with it has generally proved fatal; and in the only well-marked instance I have met with, though called in early and favoured with an opportunity of paying daily visits, I could not discover that the established and generally successful mode of practice now in use afforded at any period of the fever the smallest degree of benefit, and the patient was carried off in the prime of life, towards the end of the fourth week of the disease.

After making an apology, gentlemen, for engrossing so much of your time, I shall conclude these observations with remarking that in those cases where I have employed the tepid spongings I have done it invariably with a view of diminishing



the heat of the surface, which must be at the time considerably above that which is natural; for, as during the evaporation of fluids from the surface of the body cold is produced, or, according to the present and more accurate method of conveying our ideas, the superfluous and oftentimes morbid accumulation of caloric is abstracted, so by adopting this practice, regulated according to existing circumstances, and agreeably to the directions above recommended, I should hope that in most instances the exacerbation of fever might be diminished in violence and curtailed in duration, and hence many of the distressing accidents so often occurring during the progress of a long-continued fever would be prevented, the period of the disease frequently shortened, or its course rendered far less dangerous and alarming. To enter more at large into the *modus operandi* of a known debilitating power or agent sometimes employed in fever, which is acknowledged to be a disease of the asthenic class, would be entirely foreign from the purpose of this paper; but I would wish it to be remembered that I consider cold here as acting upon the sensations of the human body and employed during the time that an increased excitement takes place upon its surface: and until we are better acquainted with the proximate cause of fever, any theory which might be advanced would be found extremely deficient and unsatisfactory. I shall therefore decline entering any further upon the treatment of fever, and conclude these observations with recommending the cautious and limited use of cold as a new agent in addition to our present means of opposing so dreadful a malady, trusting that every member of this Society will bring forward those practical remarks upon the subject which must contribute to general information, may ultimately lead to more beneficial and important discoveries, and render us all individually more useful and more happy.

“Felix qui potuit rerum cognoscere causas!”—TANTUM.<sup>1</sup>

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<sup>1</sup> We have reproduced this paper with faithfulness as to spelling and composition, no less than as to the curious half-metaphysical ideas with which it abounds. It is interesting to see that Currie and his followers shrank from cold water in what we call “hyperpyrexia.”



## TWO CASES OF LEAD-POISONING, WITH VERY LARGE QUANTITIES OF ALBUMEN IN THE URINE.

BY E. J. SHEARMAN, M.D., M.R.C.P., F.R.S.E., ETC.

*Physician to the Rotherham Hospital.*

THE two following cases of lead-poisoning, accompanied by the excretion of very large quantities of albumen in the urine, are so interesting and unusual that I am induced to publish them, in the hope that some gentleman who has studied albuminuria more carefully than I have, may be able to account pathologically for the combination.

On the 29th November, 1873, I was called to a gentleman, aged 60, who had been seriously ill for the last twelve months, and had never been well for more than five years. I found him delirious, sleepless, with dry skin, pulse 130 to 140, very feeble respiration (30), no abnormal chest sounds, no appetite, most obstinate constipated and painful bowels, requiring very large doses of the most powerful aperients, and passing a large quantity of urine of sp. gr. 1008, half of which became solid on boiling and adding nitric acid; and blood-discs were abundantly seen under the microscope, but no casts. He was totally unable, from want of muscular power, either to walk, turn round in bed, dress or feed himself. This had been his condition for several months. He persisted in the idea that he was labouring under suppressed gout.

My first impression was that I had to combat a confirmed case of albuminuria, and for a day or two I prescribed accordingly. But I was then consulted by a lady, aged 37, who lives in the same house, and found she had been suffering,

for five or six years or more, from various remarkable spasmodic attacks, and was reduced, by their constant occurrence, from a well-made, handsome woman, to a mere skeleton. She had been scarcely able to walk for twelve months; could never relieve her bowels except by taking enormously strong medicine; was frequently seized with violent tonic spasms in the arms, fingers, legs, abdomen, and chest, as if she had been taking strychnine; and at one time, the friction of the muscles of the chest was so loud, that, combined with other symptoms, I had great difficulty in satisfying myself she had not pleurisy; her pulse was usually very quick and weak; tongue and skin dry; and no appetite. But, strange to say, this patient also had a large quantity of albumen in her urine, the sp. gr. being only 1010.

These symptoms had existed several years, in both patients, and no remedy which had ever been prescribed, except change of scene and air, relieved them: and both informed me that their appetite and health always improved in a short time after leaving home.

On reflection it occurred to my mind that these symptoms might be owing to the poison of lead; and on examining the gums of each, broad blue lines were discovered. I then ascertained that they had been drinking rain-water for more than twenty years, which was collected from the leaden roof of a large mansion into a lead cistern at the top of the house, whence it was conveyed in leaden pipes into the yard below, and finally pumped up in leaden pipes into another lead cistern in the kitchen.

I sent some of this water to a most celebrated scientific analytical chemist, and analysed it myself, and we both found lead in it; and I detected lead in the filtered urine, after boiling, in both cases, by sulphuretted hydrogen.

This was most satisfactory. I prescribed for each patient pretty large doses of iodide of potassium, with tonics and a very generous diet, and cut off the supply of lead water. The bowels began to act naturally; the appetite rapidly improved; and now, in three months, by this treatment alone, both patients are so far recovered as to be able to walk out and enjoy life.

When they began to recover, the *albumen gradually reduced*

in quantity, as I discovered *less lead in the urine*, and at last totally disappeared. But the gentleman, who is still unable to use his wrists, has occasionally a little, perhaps one fiftieth part, of albumen.

I am induced to believe, from my own reading, that albumen is very rarely found in the urine of lead-poisoning. I have seen a great many cases, but never detected it before, although I make a point of examining the urine in every case of disease, nor can I account for it satisfactorily. The quantity of albumen, for many weeks, in these cases, was quite as much as I have seen in the worst cases of Bright's disease. The only authority of any note, that I am aware of, is Ollivier, who relates, in 1863, several cases where albumen (he does not state what quantity) was found : and his opinion appears to be that the lead is deposited in the kidneys, in elimination, causing renal degeneration ; and hence albuminuria.

## AN EXPERIMENTAL OBSERVATION ON HAY FEVER.

BY PROFESSOR BINZ, OF BONN.

FROM what I have observed of recent English publications on the subject of hay fever, I am led to suppose that English authorities are inaccurately acquainted with the discovery by Professor Helmholtz, as far back as 1868, of the existence of uncommon low organisms in the nasal secretions in this complaint, and of the possibility of arresting their action by the local employment of quinine. I therefore purpose to republish the letter in which he originally announced these facts to myself, and to add some further observations on this topic. The letter is as follows.<sup>1</sup>

"I have suffered, as well as I can remember, since the year 1847, from the peculiar catarrh called by the English 'hay fever,' the speciality of which consists in its attacking its victims regularly in the hay season (myself between the 20th May and the end of June), that it ceases in the cooler weather, but on the other hand quickly reaches a great intensity if the patients expose themselves to heat and sunshine. An extraordinarily violent sneezing then sets in, and a strongly corrosive thin discharge, with which much epithelium is thrown off. This increases, after a few hours, to a painful inflammation of the mucous membrane and of the outside of the nose, and excites fever with severe headache and great depression, if the patient cannot withdraw himself from the heat and the sunshine. In a cool room, however, these symptoms vanish as quickly as they had come on, and there then only remains for a few days a lessened discharge and soreness, as if caused by the loss of epithelium. I remark, by the way, that in all my other years I had very little tendency to catarrh or catching cold, while the

<sup>1</sup> Cf. Virchow's Archiv, vol. xlv. p. 100.

hay fever has never failed during the twenty-one years of which I have spoken, and has never attacked me earlier or later in the year than the times named. The condition is extremely troublesome, and increases, if one is obliged to be much exposed to the sun, to an excessively severe malady.

"The curious dependence of the disease on the season of the year suggested to me the thought that organisms might be the origin of the mischief. In examining the secretions I regularly found, in the last five years, certain vibrio-like bodies in it, which *at other times I could not observe* in my nasal secretion. . . . They are very small, and can only be recognised with the immersion-lens of a very good Hartnack's microscope. It is characteristic of the common isolated single joints that they contain four nuclei in a row, of which two pairs are more closely united. The length of the joints is 0.004 millimetre. Upon the warm objective-stage they move with moderate activity, partly in mere vibration, partly shooting backwards and forwards in the direction of their long axis; in lower temperatures they are very inactive. Occasionally one finds them arranged in rows upon each other, or in branching series. Observed some days in the moist chamber, they vegetated again, and appeared somewhat larger and more conspicuous than immediately after their excretion. It is to be noted that only that kind of secretion contains them which is expelled by violent sneezings; that which drops slowly does not contain any. They stick tenaciously enough in the lower cavities and recesses of the nose.

"When I saw your first notice respecting the poisonous action of quinine upon infusoria, I determined at once to make an experiment with that substance, thinking that these vibrionic bodies, even if they did not cause the whole illness, still could render it much more unpleasant through their movements and the decompositions caused by them. For that reason I made a neutral solution of sulphate of quinine, which did not contain much of the salt (1.800), but still was effective enough, and caused moderate irritation on the mucous membrane of the nose. I then lay flat on my back, keeping my head very low, and poured with a pipette about four cubic centimetres into both nostrils. Then I turned my head about, in order to let the liquid flow in all directions.



"The desired effect was obtained immediately, and remained for some hours; I could expose myself to the sun without fits of sneezing and the other disagreeable symptoms coming on. It was sufficient to repeat the treatment three times a day, even under the most unfavourable circumstances, in order to keep myself quite free.<sup>1</sup> There were then no such vibrios in the secretion. If I only go out in the evening, it suffices to inject the quinine once a day, just before going. After continuing this treatment for some days the symptoms disappear completely, but if I leave off they return till towards the end of June.

"My first experiments with quinine date from the summer of 1867; this year (1868) I began at once as soon as the first traces of the illness appeared, and I have thus been able to stop its development completely.

"I have hesitated as yet in publishing the matter, because I have found no other patient<sup>2</sup> on whom I could try the experiment. There is, it seems to me, no doubt, considering the extraordinary regularity in the recurrence and course of the illness, that quinine had here a most quick and decided effect. And this again makes my hypothesis very probable, that the vibrios, even if being no specific form but a very frequent one, are at least the cause of the rapid increase of the symptoms in warm air, as heat excites them to lively action.

"Yours, &c."

I should be very glad if the above lines would induce medical men in England—the haunt of hay fever—to test the observation of Helmholtz. To most patients the application with the pipette may be too difficult or impossible; I have therefore already suggested the use of Weber's very simple but effective nose-douche. Also it will be advisable to apply the solution of quinine *tepid*. It can, further, not be repeated often enough that quinine is frequently adulterated, especially with cinchonia, the action of which is much less to be depended upon.

<sup>1</sup> There is no foundation for the objection that syringing the nose could not cure the asthma which accompanies hay fever; for this asthma is only the reflex effect arising from the irritation of the nose.—B.

<sup>2</sup> Helmholtz, now Professor of Physics at the University of Berlin, is, although M.D., no medical practitioner.—B.

Dr. Frickhöfer, of Schwalbach, has communicated to me a second case in which hay fever was cured by local application of quinine (cf. *Virchow's Archiv* [1870], vol. 51, p. 176). Professor Busch, of Bonn, authorises me to say that he succeeded in two cases of "catarrhus æstivus" by the same method; a third patient was obliged to abstain from the use of quinine, as it produced an unbearable irritation of the sensible nerves of the nose. In the autumn of 1872 Helmholtz told me that his hay fever was quite cured, and that in the meantime two other patients had, by his advice, tried this method, and with the same success.

## Reviews.

*Untersuchungen über den fieberhaften Process und seine Behandlung.* Von Dr. H. SENATOR, Docent an der Universität in Berlin. 8vo. pp. 208; 1873. Berlin: Hirschwald. London: Williams and Norgate.

THESE researches upon the febrile condition and its treatment are of great intrinsic value; and they come very opportunely at the present moment, as they afford us a good excuse for reviewing the general state of our knowledge regarding the nature and treatment of pyrexia. We doubt whether the general profession is sufficiently alive to the interesting position in which these questions now stand, and we propose to attempt some elucidation of the principal points at issue.

Fever, or pyrexia, as a general state linked to many different conditions of the bodily organs, is once again assuming the importance of which it seemed to be temporarily deprived by the striking progress of knowledge respecting local morbid changes which was effected during the first half of the present century. A marked elevation of the bodily temperature, persisting, though with diurnal fluctuations, through many days or weeks, is acknowledged as a phenomenon which has an importance of its own, whatever may be the condition of particular organs with which it may happen to be connected in any individual case. Of this fact pathologists have for some years past become increasingly convinced; but undoubtedly the most decisive impulse to medical opinion has been given by two events. The first of these is the discovery, due to Parkes more than to any other observer, of the very general tendency, in acute diseases with considerable and persistent elevation of temperature, to a production and elimination of urea which obviously depended on something quite different to the normal vital processes of tissue-exchange. The full significance of this fact was but tardily appreciated, owing to the slowness with which the modern doctrines of nutrition have been accepted by the profession. A febrile excretion of 600, 800, or 1,000 grains daily of urea was far from assuming its true importance in the

eyes of pathologists so long as they clung to the belief that the urea-discharge of health was the outcome mainly of destruction of muscular tissue in the ordinary vital work. The second great impulse to recent changes in fever doctrines has been caused by the remarkable facts brought to light by the results of cold-water treatment of pyrexia so happily revived during the last thirteen years. It is singular that the essay of Brand "On Hydrotherapy of Typhus (typhoid)" was published in 1861, and the invaluable work of Parkes "On the Urine" in 1862. Those two years are memorable landmarks in the progress of febrile pathology: from them we date an entirely new kind of advancement towards the solution of the mysteries of pyrexia. The work now before us is in every respect a worthy continuation of the important investigations of which Parkes and Brand may be said to have been the respective originators.

We shall deal, first, with the more striking and simple part of modern fever doctrine and fever treatment, viz., the effects (as they are now understood) of superheating of the blood, and the treatment which is directed to the simple physical removal of the superabundant caloric which is working a direct mischief in the organism and may even prove fatal by its own unaided influence. It is not necessary to recall to our readers many facts which have been repeatedly stated in the pages of this journal. It will be remembered that a constantly accumulating testimony has shown that when the blood remains for more than a very limited period at a temperature much beyond the normal, certain lesions of the tissues are uniformly induced. If the mean daily temperature, for instance, be as much as 103° Fahr. for ten, fifteen, or twenty days together, that fact of itself determines a number of functional affections of the most serious kind, and contributes, probably, in an important degree to fatty and other degenerative changes of the organs. On the part of the nervous centres the evil influence is confessed by the occurrence of delirium, coma, paralysis, sometimes going on rapidly to extinction of life; on the part of the muscular tissues there is an increased wasting and feebleness, and where this extensively affects the heart or the respiratory muscles, life may be cut short by the sudden failure of one or other of those organs.

Concerning the treatment of this condition of danger from mere excess of heat, there is no longer any difference of opinion, except as to the question of degree, between those who have really investigated the subject. The mass of the profession (including some of its nominal leaders) is, indeed, as yet far enough from putting in practice the treatment which is indisputably the best, even in the cases where it can be shown to be not merely the best method but actually the only one which

offers the slightest chance of a favourable issue. But among the real leaders of medical thought and experience it has been settled beyond the possibility of question (as the paper of Professor Behier in our February number clearly shows), that there is only one way of dealing with excessive bodily heat which is itself dangerous to life, viz., the abstraction of caloric from the surface of the body by cold baths or other simple physical methods. After the striking services which this method has been shown by Dr. Wilson Fox to render, even in the apparently desperate circumstances of extreme rheumatic hyperpyrexia, there can be no longer any doubt upon this point. At the same time it may be worth while to consider an indication pointed out by Senator, whether we accept or reject his particular way of fulfilling it. Senator belongs to that school which regards the diminution of temperature by the cold bath not merely as a simple physical phenomenon, but as produced in part by a stimulation of the superficial nerves. So important does he consider this latter action to be, that he proposes to supplement it by an additional method. Before the bath, he applies very large mustard poultices, fastened on so firmly that they can be retained while the patient is in the water; his obvious intention being to produce a reflex dilatation of the vessels of the skin, and thus an increased discharge of heat from the surface.

Whether this view will ultimately prove to be just we cannot say; but we cannot help feeling with Professor Behier, that Senator's proposal savours of impractical refinement. For the higher ranges of febrile heat, it certainly appears to us that the purely physical abstraction of heat is the one sufficing remedy, and that attention is prejudicially distracted when it is turned to other methods.

Although it is no longer disputed that, when present in any great excess, heat is by far the most dangerous element of the febrile process, it is otherwise with those medium and lower ranges of temperature which would still be universally called febrile. If for example we take the very common case of a severe typhoid fever, with temperature in the third week fluctuating between  $103^{\circ}$  and  $105^{\circ}$ , morning and evening, in what relation are we to suppose that the febrile heat and the degenerative changes which we should probably find in the muscles, the liver, &c., if the patient died, stand to each other? Pathologists are far from accepting *en bloc* the doctrine of Liebermeister, which would ascribe these degenerations almost exclusively to the influence of the heat itself. As Senator himself points out, supporting himself by numerous quotations from authors, there is no such agreement between the intensity and duration of fever and the amount of anatomical change as would support



this exclusive theory. Here, then, we begin to touch an entirely different order of considerations, which must form the theme of the second part of our observations.

No suggestion was ever propounded in medicine more pregnant with future illumination than that of the increased destruction, in fever, of the nitrogenous tissues; and certainly no country has on the whole done so much to develop this important line of research as our own. The name of Parkes (which must always hold the first place) must be joined in this respect with those of Buchanan, Ringer, Squarey, and others who forced this topic on the consideration of the profession at a time when its true importance was very little perceived; and we may here notice with some regret, that the distinct precedence of the heightened urea-discharge to the rise of temperature, which is one of Senator's most important points, is not acknowledged by him as having been previously observed by Ringer. However, we have no wish to quarrel with an observer who has done so much, both in the way of personal observation and in collecting the experience of others respecting this exceedingly important topic of tissue-change in the febrile state. We must ask to be forgiven if we tax our readers' patience by a literal transcript of the numerous conclusions at which the author arrives.

"1. In the majority of diseases commencing with a high febrile temperature, there is no single and equable increase of tissue-exchange, but a disproportionately large destruction of albumen which leads to an increased urea-discharge.

"2. The increased formation and discharge of urea takes place from the commencement of the febrile process, and even before the beginning of the rise of temperature. This increased elimination amounts, on an average of the whole febrile period, to double what occurs in the absence of fever. The conditions are more unfavourable to the elimination of urea when formed in fever than in health.

"3. For this reason, and because albumen usually undergoes change also in other ways, the albuminous loss is yet greater than is represented by the urea, and is, in general, much more than doubled.

"4. The conditions for the discharge of carbonic acid are more favourable in pyrexia than in health: under propitious circumstances it is increased in the daytime by 30 or 40 per cent.; in the night it is probably, as in health, at a lower level than in the day. The formation of carbonic acid in fever is thus, at most, only increased by less than 30 or 40 per cent.

"5. It follows from this that the fevered organism is relatively poorer in nitrogenous constituents (albumen), but richer in non-nitrogenous and carbonaceous substances (fat).

"6. Of the nitrogenous constituents, those are most largely destroyed which are rich in potash and blood-pigment: for instance, the blood-corpuscles, and then the muscles. The increased destruction of the red blood-cells, which are carriers of oxygen, produces fatty parenchymatous degenerations, just like those poisons which interfere with oxidation.

"7. Since the febrile organism cannot take up so much oxygen as a feverless organism similarly nourished, it also cannot furnish so many products of oxidation. But since the increased formation of urea has already used up larger quantities of oxygen, the other products of oxidation, such as carbonic acid, cannot be furnished in larger quantity.

"8. In the absence of oxidation, the only possible increase among the final products of tissue-exchange is that of the water, from synthesis and dishydration.

"9. The quantity of urine is generally regulated, in fever, by the supply of liquid; but it stands in a more unfavourable relation to the latter than in health, for a relatively smaller quantity of the fluid swallowed is discharged as urine than in the feverless state under similar circumstances.

"10. The quantity of watery evaporation is increased in fever, and to a greater degree than that of the expired carbonic acid; a relatively larger portion of the water drunk is therefore got rid of by evaporation.

"11. The relation of the total water-discharge to the water-formation in fever, over and above the ordinary loss, is not yet distinctly made out. As happens in health, the water supply may be larger, yet water may be retained in the body. These varying relations of the supply and discharge of water are the principal causes of the fluctuations in body-weight in fever.

"12. The critical increase of urea-formation probably depends, not on incomplete oxidation of nitrogen, but on various other causes.

"13. The waste of heat at the beginning of a fever is not increased, but diminished, during the shivering; at the height of the fever it is increased on the average, in the daytime to about 70 to 75 per cent., but considerably more at the critical defervescence.

"14. The greater part of the waste of heat depends, in pyrexia as in health, upon conduction and radiation; but in the critical defervescence upon evaporation.

"15. The excess of heat in the febrile organism cannot be explained by the tissue-destruction which is expressed by the final products—urea and carbonic acid.

"16. This excess has various causes, of which the following can, so far, be assigned with certainty:—(1) The consumption

of the tension-force which is available for work in the healthy organism; (2) the increase of heat in the pyrogenetic stage preceding the fever heat. Besides this, there are perhaps, in the increased conversion of albumen to urea, and in unknown processes connected with the formation of water, sources for the increased production of heat.

"17. The skin, in fever, has not lost its power to regulate the bodily temperature by its fluctuating contents of blood and heat; but its activity is diminished by the fact that, under the influence of the causes of fever, there is an abnormal excitability and stimulation of its vessels, whereby the latter, from the first development of fever, are either generally or locally contracted, and thus hinder the regulation of the heat-excess.

"18. The elevated temperature in fever is also favoured by the perverted relations between the abnormally increased formation and the less increased discharge of heat. In this way the discharge of heat may be always greater than normal at the height of fever, and occasionally even greater than the febrile heat-formation. This perverted relation does not occur equally in every phase of the fever, and necessitates the occurrence of a pyrogenetic stage before every burst of fever heat, and an exactly opposite process in the stage of defervescence.

"19. The pyrogenetic stage is most strongly expressed in the febrile shivering, the defervescent stage in the crisis; the same processes occur, however, only less stormily, in every kind of development and decline of the fever, and are also expressed, though more weakly, during the febrile stage in the fluctuation of increase and diminution of the waste of heat.

"20. The absence or presence of sweat does not depend on the febrile process in itself, but on the nature of the febrile disease, or on peculiar causes, and has no relation to the regulation of heat. It is probable that in many febrile diseases the necessary stimuli to perspiration are absent, or are hindered in their operation; while in other diseases, and at the occurrence of the critical sweat, the opposite conditions prevail."

Such are the conclusions at which Senator arrives; and the reader, whether he be inclined to accept them all or not, will confess that they offer a broad and most suggestive view of the pyrexial state. Perhaps the most valuable service that they render is the light which they throw on the possible independence of the pyrexial temperature of positive destruction of tissue. Senator states very strongly that the tissue-destruction (as evidenced by urea and carbonic acid discharges) is not sufficient to account for the amount of fever heat. We assent entirely to the statement, and we shall only render it more important if we put in a preliminary caveat, to the effect that

Senator decidedly underrates the normal amount of urea discharge, and that the "doubled or trebled" rate of urea-discharge which he mentions as occurring in pyrexia does not mean, as might be supposed, the discharge of 1,000 or 1,500 grains of urea in twenty-four hours by an adult suffering from fever. On the other hand it must be remembered, as Senator himself points out, that the urea is not the full measure even of the albuminous destruction; and that, further, there is all the destruction of fatty tissues (frequently so striking and extensive) to be considered as a source of heat-production. The best proof, however, that tissue-destruction is not the only source, nay, that it *may* even hardly be a source at all, of febrile heat-excess, is to be found in such cases as those related in the March number of this journal<sup>1</sup> (and which appear to be not so very uncommon), in which a very high pyrexia is accompanied by a quite low urea-discharge, and an absence of any considerable emaciation either of the muscular or of the fatty tissues.

It is most important that attention should be drawn to a cause of febrile heat which, though not overlooked by Senator, is ignored by most writers, and unquestionably is never thought of by the majority of practitioners. We refer to that removal of the *tendency for force to become latent* which takes place when, as in pyrexia, the usual quiet building up of tissue comes to a sudden end. In ordinary tissue-building a large amount of force becomes latent; in fever that same force is set free under the form of heat. If we add to this the collection of heat within the body in the early febrile stages (when there is little or no heat-waste), and the obscure chemical processes connected with water-formation, we have enough to account for a large heat-production, even independently of the destruction of nitrogenous and fatty tissues.

It is very surprising to us that Senator should have been content with such feeble and insufficient trials of the action of alcohol in pyrexia, considering the pointed way in which he describes the destruction of tissues which accompanies that state. The chief reason perhaps is, that his febrile experiences seem to have lain more in the direction of relapsing fever than in any other. It seems very plain that there is no more important duty now incumbent on pathologists and physicians than to try every possible means by which we may strike at the root of that excessive combustion of tissues which (in any fever which is below the degree of imminently dangerous hyper-pyrexia) is one of the phenomena that we have most to dread. We shall hope to see this able observer entering with great zeal upon a line of inquiry which he is so amply qualified to carry out with solid results.

<sup>1</sup> "On Tissue-destruction in the Febrile state." By Dr. Austie.



## Clinic of the Month.

**Discussion on Cancer.**—The chief subjects of clinical interest in the journals of the present month have been the discussion on pyæmia in the Clinical Society, and the discussion on cancer in the Pathological Society. The latter discussion was opened by Mr. Campbell de Morgan. Mr. De Morgan defines cancer to be a malignant growth, consisting of a delicate fibroid stroma, containing within its meshes aggregated but not coherent cell-elements—cells, nuclei, or granules—generally non-uniform, though often similar to natural cell-elements. He limits the term “cancer” to those forms of disease which are specialised as medullary cancer, scirrhous, epithelioma, and rodent cancer; and in regard to its origin, the view he is disposed to hold is that, though essentially local, there is in some, and perhaps in all cases, a predisposition to the disease, which may possibly be distributed through the system, but which more probably has its seat in some among the tissues of the body. He therefore questions the evidence of there being any special disease in the blood which either alone produces cancer or co-operates with tissue specially fitted for the development of the disease. He adduces as arguments against the constitutional or blood-contamination theory, the good health of the patients previously to the discovery of a cancerous tumour; the possibility of its non-return after removal; the recovery of wounds and other lesions in those actually suffering from cancer; the occasional wasting away of a cancerous tumour; and its tendency to attack women. Mr. Simon remarked that, looking on the one hand at the hereditariness of cancer, and on the other at the insusceptibility of bodies generally to the inoculation of cancer, it is necessary to assume that a certain specific predisposition is required. In addition, he thinks we must recognise that the essence of the disease is the development in some one spot of a specific impregnative influence; but whether this local infection is a local neo-genesis, or is a contagium *ab extrâ*, is a question on which it is not safe to speculate. Mr. Hutchinson followed the same line of argument, and con-



tended for the local origin of cancer, and showed how it was possible by neglect and irritation of simple sores to "cultivate cancer." Sir James Paget held that we must look at the same time for a local and a constitutional origin of cancer. He admits that there are many cases where the disease appears to have a local origin, or where at least there must have been so little constitutional predisposition to cancer that it required some tissue to be specially prepared through a course of years before it could either become cancerous or the seat of cancer; but, on the other hand, he thinks there are certain things in the history of cancer in which it is impossible not to discern a very large predominance of the constitutional element. First, there is the manner of its inheritance, affecting in the offspring not the same tissue always, but other and very various organs and tissues; secondly, there is the fact that after local injuries there is a deviation from the ordinary mode of repair which can only be attributed to some constitutional peculiarity or taint; and thirdly, there is the tendency to recur after removal, which he thinks is very much greater than is commonly admitted—so high in the case of the breast in women, that 500 cancers recur to one that does not. The reason of cancer being more frequent in women than in men, he attributes to the circumstance that cancer is eminently a disease of degenerated tissues, and that women have in the breast and uterus organs that undergo degenerative changes at a comparatively early period. Sir James considers that in regard to treatment there is more hope in cancer being a constitutional than in its being a local disease, for attempts at removal by the knife or caustics have persistently failed, whilst the analogy of syphilis would lead to the probability of there being some remedy which may arrest its development and effect a cure. Mr. Arnott argued in favour of the local origin of cancer, but admitted that some part must be played by the constitution: he thought the malignancy of certain tumours may be explained by their position, so that in proportion as that position is subjected to the conditions of warmth, of moisture, of great vascular supply, of free lymphatic connections, and of occasional exalted functional activity, do we find that the tumour is likely to become malignant. Mr. Arnott further pointed out how mobile were the elements of even the hardest tumours, and with what facility they might be transmitted from one part of the body to another. Sir W. Gull argued in favour of cancer being a local disease, on the ground that it had seats of election: he thinks that if the part affected could be taken away, the patient would be rid of the cancerous tendency in the particular part or place; and he points to the favourable results of the removal of the eye in medullary sarcoma of the retina. (*Lancet*, March 1874.)

**Röthelen or German Measles.**—Dr. Robert Liveing, in a clinical lecture on this affection, records four cases, and then proceeds to make the following remarks:—1. The premonitory fever in German measles is generally mild, and resembles in many subjects, though not in duration, that of common measles. There are more or less pain in the limbs, slight shivering, sore throat, and often, though by no means always, coryza, redness of the conjunctiva, and sneezing. The characteristic feature of it, however, is its duration, which is seldom more than twenty-four hours, whereas in ordinary measles it is from three to four days. 2. The character of the eruption when it first appears is almost always described as “measly,” that is, in small reddish patches. In the first instance the rash consists of small rounded collections of minute red papules, which after a time coalesce and form large irregular patches, just as in measles, but with apparently less tendency to become of a horse-shoe or crescentic shape. After a time the patches may all unite, and then the skin becomes to the naked eye of a uniform red colour closely resembling that of scarlet fever. The rash lasts longer than the rash of either measles or scarlet fever—from four to ten days. Its disappearance is followed by a desquamation of branny scales, though this is but slight in mild cases. 3. Amongst the most constant symptoms of this fever is the persistent though not generally severe sore throat. The tonsils are red and swollen, and remain in that state usually for some days after the rash has faded; indeed, the sore throat is often the last symptom to disappear. Unlike that often associated with scarlet fever, it rarely leads to ulceration. 4. The presence of albuminuria is of not unfrequent occurrence, and occasionally leads to acute dropsy. 5. The disease propagates itself, and never leads to the production of either scarlet fever or measles in others, which is conclusive against the idea of its being a mild type of either. 6. It gives no protection to either measles or scarlet fever, nor do these diseases give any protection to those exposed to German measles. The existence of this disease perhaps accounts for some supposed errors of diagnosis, when a patient said by the first practitioner who sees him to be suffering from measles is pronounced by a second, under whose notice he comes at a later period, to be suffering from a slight attack of scarlatina. (*Lancet*, March 14, 1874.)

## Extracts from British and Foreign Journals.

**The Curability of Phthisis.**—Dr. Massini has published an essay on this subject in the *Deutsche Archiv für Klin. Med.*, in which he remarks that under the influence of constitutional predisposition, which is usually hereditary, but may be engendered by defective hygiene or contagion, various organs, and especially the lungs, become inflamed, the final result being caseous degeneration and suppuration. In the case of the lungs, this result is most frequent in catarrhal and in the so-called interstitial pneumonia, in catarrh of the apices, in croupous inflammation of the upper lobes of the lungs, and in hæmorrhage from the lungs: all these forms are included under the term caseous pneumonia, or phthisis pulmonum. The affections of the larynx and intestine which are of such frequent occurrence in phthisis are only concomitant affections, and are not to be regarded as primary phthisis of these parts. Miliary tuberculous deposits in the other organs of the body are also to be regarded as secondary in their nature, but may be attended with such stormy and violent disturbance of the system as to resemble a febrile infectious disease like typhus: or, as in the case of tuberculosis of the cerebral membranes, may resemble acute inflammation of the brain. From the statistics of post-mortem examination of phthisical patients, two-thirds of all cases prove fatal from uncomplicated caseous processes, and only one-third are generally tuberculous. Recovery from miliary tubercles is very rare, indeed their curability has not hitherto been demonstrated. In the treatment of phthisis the prophylaxis is of great importance. The treatment of catarrh and inflammation of the thoracic viscera should be early and carefully undertaken. Where caseous inflammation and suppuration are already present, if febrile symptoms indicate the continuance of the process, the patient should be confined to bed, quinine and digitalis should be administered, and the application of cold douches and spongings be tried. Inhalations of carbolic acid may also be used to limit the suppurative process. Arsenic has often proved beneficial in such cases by diminishing the fever and promoting

nutrition. When the febrile symptoms are subdued, the treatment must be directed to strengthening the powers of the patient. This may be accomplished by the milk-cure, cod-liver oil, and change of air. When the patient suffers from chronic catarrh, Massini recommends Italy as a winter residence; but when cheesy deposits are already present, the Alpine regions, under the care of an experienced physician, are the best for the patient. (*Der Praktische Arzt*, xiv. 12.)

**Quinine in Croupous Pneumonia.**—Dr. Jürgensen, in one of Volkmann's Clinical Lectures, translated by Dr. Kirchner, observes that quinine, when administered in proper time and quantity, diminishes the temperature for at least twelve hours. The minimum,  $1^{\circ}5$  to  $2^{\circ}5$  less than the temperature at the beginning, takes place from five to seven hours after taking the remedy. The falling off and rising again take place in a pretty straight line, as Jürgensen has himself shown by measurement, every five minutes. A good deal depends on the manner of giving the quinine. Prof. Jürgensen gives it in moderately severe cases to adults according to the following formula:—

Quiniae sulphatis, ʒss  
Acid. hydrochloric dil. q.s. ad solut.  
Aquæ distillat. ʒjss

To be taken at once in the evening, between 6 and 8 o'clock. To children up to the fifth year he gives about  $\frac{1}{2}$  of a grain for each year; after that age from  $7\frac{1}{2}$  to 15 grains. These doses may however be exceeded without harm, so that in a severe case, in an adult who is vigorous, 75 grains may be given. The quinine sometimes has an emetic effect, which may be prevented by letting the patient bend over as soon as the dose is taken, to allow the generally profuse salivation to run off. Small pieces of ice also tend to prevent the emetic effect, but if rejected the dose should be repeated a second or even a third time. With the temperature the pulse diminishes, but at the same time remains full and strong. In extreme cases, when the stomach cannot tolerate the quinine, it may be administered by an injection with a little tinct. opii. Under any circumstances twenty-four or thirty-six hours should intervene between two doses of the remedy. (*St. Louis Med. and Surg. Journal*, vol. x., No. 12.)

**Relation of Renal Apoplexy with Cerebral Hæmorrhage.**—Dr. Auguste Ollivier remarks that the influence of chronic affection of the kidney on the production of cerebral hæmorrhage is well known, but in the present essay he proposes to consider the converse fact, namely, the influence of cerebral hæmorrhage on the production of certain renal lesions.



He proceeds to give a series of cases that have fallen under his observation, amongst which are the following :—1. A man aged 74, attacked with apoplexy, with right hemiplegia of the limbs and face. In twenty minutes after the attack the urine was found to have become albuminous; death took place on the fourth day. On post-mortem examination effusion of blood was found to have occurred under the meninges, and a clot had formed in the left optic thalamus and corpus striatum, which had burst into the corresponding ventricle, and had made its way into the middle ventricle and the opposite lateral ventricle. The cerebral peduncles, cerebellum, pons, and medulla oblongata were healthy. Both kidneys, but especially the right, were found to be congested, 2. An attack of apoplexy in a man aged 68, with hemiplegia of motion and sensation on the right side; ten minutes after, coma. The urine was normal up to the time of the appearance of the coma, but then began to contain albumen. Death occurred in the course of nine hours. The autopsy showed a large hæmorrhage into the white substance of the left cerebral hemisphere, which had burst into the ventricles, and even entered the upper part of the Aqueduct of Sylvius; both kidneys were congested, the right highly so, and with an ecchymotic spot upon it. M. Ollivier reports several other similar cases. He then proceeds to give the details of numerous experiments he has recently performed on rabbits, showing, in accordance with the statements of Bernard, Brown-Séquard, and others, that puncture of the floor of the fourth ventricle is almost constantly followed by the appearance of albumen in the urine. The cause of this is not, however, quite clear; some, with Schiff, considering it to be due to paralysis of the vaso-motor nerves; others, with Brown-Séquard, to be due to spasm of the veins, or of both arteries and veins, causing engorgement and rupture of the capillaries.

M. Ollivier, in conclusion, points out that albuminuria of cerebral origin is of more frequent occurrence than is generally admitted, but that our present knowledge does not enable us, from the existence of albuminuria in cases of cerebral hæmorrhage, to localise exactly the seat of lesion. At the same time, where the symptoms of lesion of the medulla oblongata are deficient in an apoplectic patient, it may be said that the presence of albumen in the urine indicates either a hæmorrhage situated at the base of the brain, or an extensive hæmorrhage elsewhere exerting compression on the base. In all instances it must be regarded as a very serious symptom. (*Archives Générales de Médecine*, February 1874.)

**Treatment of Uterine Hæmorrhage during the Puerperal Period.**—Dr. Snow Beck discusses the subject of the



treatment of uterine hæmorrhage at this period by means of styptics. He shows that though at first only used as a *dernier ressort* in cases of great peril, and where other remedies were said to have been employed and had failed, yet of late it has been employed in all cases of post-partum hæmorrhage to the exclusion of all other remedies. It has even been employed to prevent the occurrence of hæmorrhage which had been anticipated from the experience of former labours in the same individual. So far, he continues, as we at present know, no reasonable objection can be urged to the employment of these strong styptics, provided the one essential condition be attained—the complete and permanent contraction of the gravid uterus. The danger consists, as shown by the examination of the fatal cases that have occurred after their use, in only partial contraction being produced, by which, although the hæmorrhage is arrested, yet the veins and sinuses remain open and permit the perchloride of iron to be taken up and conveyed into the general system. Blood-poisoning commonly then supervenes in the course of a few days, ending in the almost inevitable death of the individual. Dr. Beck asks whether we are justified in the use of substances of such doubtful, not to say dangerous power, when other means of equal or greater efficacy may be employed, and to which no danger attaches. He demurs to the view that because hæmorrhage has not been arrested when the patient has been deluged with water or the vagina filled with ice, it cannot be stopped when cold is properly applied, as by the introduction of ice into the cavity of the uterus and renewed as often as the pieces melted away, and this combined with effective pressure from the abdomen by the aid of a strong assistant. Dr. Beck then refers to a practice he has for a short time past adopted. Reasoning upon the experience obtained by the injection of the perchloride of iron, and considering that the chief danger appeared to arise from the serious effects of this substance when conveyed into the general system, the question was naturally suggested, whether some stimulating astringent could not be thought of which would excite the uterus to contraction when injected into the cavity, but which would not be followed by such serious results, supposing a portion did become taken up into the general system. In large towns it is generally possible to procure a supply of ice at all seasons of the year; but in the country, it appeared desirable to have some agent which could be generally obtained, and which might be resorted to, as an additional means of inducing contraction. It occurred to Dr. Beck that ordinary vinegar might fulfil these indications; and with this view he has employed it on six or eight occasions, in the proportion of one part of vinegar to two or three parts of water, with very good effect

and encouraging results. He does not mean to say that this is the most suitable agent that can be thought of as an additional means of inducing contraction; for it is not improbable that when attention is directed to the point, a more certain and more efficient means may be found out. But it has certainly acted very well so far; and it is not probable that it will be followed by the same serious consequences as the solution of the perchloride of iron. He also suggests the desirableness of using a weaker solution with cold water after every labour, in conjunction with the administration of the ergot of rye, to induce complete and permanent contraction of the gravid uterus; and thus to obviate as far as possible those distressing puerperal complications which too frequently follow childbirth. (*Obstetrical Journal*, February 1874.)

**Treatment of Hæmorrhoids by Linear Cauterisation of the Anus.**—M. Voillemier, after discussing the various methods practised for the cure of hæmorrhoids, points out their dangers and inconveniences, and proposes as a safe and effective remedy the employment of actual cautery. Reflecting on the fact that many people have large internal hæmorrhoids, from which they experience no inconvenience, and of which they do not complain until they escape and form an external swelling, M. Voillemier conceived the idea of leaving the hæmorrhoidal tumours themselves alone, and simply preventing their escape through the sphincter. With this object in view, he chloroforms the patient, and places him on the side of the bed, as in the operation for fistula ani. The neighbouring parts are protected by large pads of cotton-wool and collodion, whilst the parts exposed are frozen by ether vapour. Two small cauterics of axe shape are then heated and introduced into the anus to the depth of one-third of an inch, making four lines of cauterisation,—in front, behind, and on either side: cold-water dressing is then applied. Under the influence of the congestion caused by the cauterisation the hæmorrhoidal tumour appears during the first day or two even more voluminous than before; but it soon ceases to be painful, and ends by being thoroughly retracted. M. Voillemier has operated on forty-three patients in this manner without the occurrence of any accident. The time necessary for healing has never exceeded a month. (*Gazette Hebdomadaire*, 1873.)

**Fibroid Uterine Tumour treated with Chloride of Ammonium.**—Dr. Hatch, of Sacramento, reports a case of fibroid uterine tumour of the left side. The patient suffered also from severe neuralgia of the supra-orbital and temporal regions. For the relief of the latter complaint, chloride of ammonium was prescribed in 15-grain doses, every two hours.

It was continued for several months, the dose being sometimes decreased, when a very decided diminution in the size of the tumour in the abdomen was remarked. The use of this medicine was steadily persisted in, and before the end of the year the tumour seemed to have entirely disappeared. (*Pacific Medical and Surgical Journal*, vol. vii., No. 6.)

**Treatment of Poisoning with Chloral.**—Dr. Albert Erlenmeyer discusses the best method of treating patients who, either by inadvertence or idiosyncrasy, have taken too large a dose of chloral. The symptoms of the toxic influence of this substance are—collapse, diminution of the frequency of respiration, which has been observed to be reduced to four in a minute; injection of the conjunctiva, contraction of the pupil, blueness of the lips, dropping of the lower jaw and retracted tongue, whilst the pulse is in the early stage strong and slow, but subsequently becomes frequent and feeble, and ultimately scarcely perceptible. In more protracted cases, the face becomes pale, there is a tendency to fainting and vomiting, rigors, disturbance of voluntary movements, weakness of the lower limbs, and cramps in the calves of the legs. Erlenmeyer recommends, first, that the chloral should be removed from the stomach by emetics or the stomach-pump, or be much diluted with water, tea, or coffee; secondly, that artificial respiration should be maintained; and thirdly, that some antidote should be given. Erlenmeyer doubts the value of strychnia as recommended by Liebreich, since, although chloral is useful as an antidote to strychnia, it by no means follows that strychnia should be an antidote to chloral; for we find that morphia is an antidote to atropine poisoning, but atropine is not an antidote in poisoning by morphia. He thinks musk might be tried, but is inclined to place most reliance on liquor ammoniæ, subcutaneously injected. As a last resource, transfusion may be adopted. (*Der Praktische Arzt*, Band xiv. 11.)

**Injection of Hydrate of Chloral for the radical Cure of Varix.**—Prof. Luigi Porta, continuing his experiments commenced in 1870, when he first observed the coagulating power of chloral on the blood, has discovered that it constitutes an excellent means of effecting the radical cure of varices. He has treated fifteen cases of varix of the leg by its subcutaneous injection with success. He began by injecting 15 grains, but gradually reduced the quantity to 7, and ultimately to 5 grains, and injected this quantity, contained in one of Pravaz' syringes, into numerous points, for the action of the remedy is greatest when it is injected at many separate points. Each of these injections forms a coagulum, and the patient should remain at rest for a few days to prevent slight phlebitis. The coagula undergo absorption and the veins atrophy, or if they remain

patient they cease to be varicose. Accidents are very rare, and in all instances where they have occurred have been very trifling. They are : (1) Softening of thrombus, usually of only a transitory nature, which does not interfere with the cure. (2) Phlebitis, which is also slight and soon subsides. (3) Limited abscess, which does not interfere with healing and may perhaps depend on the entrance of a little of the fluid into the connective tissue, and which might perhaps be prevented by making the piston act as an aspirator before extracting the needle from the veins. (4) Gangrene has been observed in the form of a circumscribed eschar the size of a bean or a threepenny-piece, especially in old people, where it was probably caused by the contact of the solution of chloral with the connective tissue, but this has not interfered with the recovery. It is to be presumed that this mode of treatment is not limited to varices, but might be applied to other affections of the veins, such as varicocèle ; indeed, Porta reports one case of cure of this disease by injection : and in cases of aneurismal varices and hæmorrhoids, Professor Coletti, in commenting upon these statements, remarks that for some months, in the wards of the hospital at Milan, varicose ulcers have been successfully treated by the application of a solution of hydrate of chloral containing one part to four of water ; recovery took place with great rapidity. (*Annale de Chemica*, Giovanni Polli 1874, p. 353.)

**The Transmission of Syphilitic Contagion in the rite of Circumcision.**—Dr. Taylor, of the New York Dispensary, remarks that the question of the possible occurrence of syphilis in the religious rites of circumcision is yet in a wholly unsettled state, and though mentioned by some authors as if it were an admitted fact, it cannot, upon a consideration of all the facts known, be regarded as fully proved. Early in the present year the attention of the Board of Health of New York was called to the existence of four cases in which it was suspected that syphilis had thus been communicated, and Dr. Taylor was appointed to investigate them. A Hebrew circumcised four Jew children, who, previously healthy, were subsequently affected with phagedenic ulcerations of the penis, by lesions of the skin and lymphatic ganglia, accompanied in three of the cases by exhaustion which terminated in death. The details of the cases are very fully given by Dr. Taylor, and this gentleman, after a careful review of all the circumstances, arrives at the following conclusions:—1. That in the Jewish rite of circumcision there is a possibility of the occurrence of syphilis. 2. That the contagion is most likely to be communicated in the act of sucking the wound, if the mouth contain a syphilitic fluid, and that perhaps it may occur by means of instruments soiled by



syphilitic blood. 3. That the chances of such contagion are rendered greater by the performance of the operation by irresponsible non-professional persons. 4. That the operation of sucking should be wholly abolished, and that if a styptic solution of any kind be used, it should be poured upon the wound rather than gurgled upon it from the mouth of the operator. 5. That in no instance should two or more children be thus operated on consecutively without a thorough cleansing of the instruments and utensils used after each operation, and that in every instance the greatest care should be taken in cleansing the instruments. 6. That the performance of the rite should be absolutely confined to responsible and educated persons, either a physician alone being selected, or a physician assisting as officiating rabbi, or a circumciser of recognised merit. 7. That under these circumstances accidents of any kind are reduced to a minimum. (*Pamphlet*, New York, 1873.)

### **Treatment of Mucous Polypus of the Velum Palati.**

—A case is recorded of this affection by Dr. Méplain, of Moulins, in which a young man, 30 years of age, suffered from a small tumour of bright red colour, soft consistence, not pulsating, slightly pedunculated, situated at the junction of the hard and soft palate, a little to the left of the median line. Its duration had been about three weeks, and it had grown rather rapidly from the size of a lentil, when it was first noticed by the patient, to that of a bean, when Dr. Méplain first saw it. It interfered with deglutition and speech, and was liable to frequent hæmorrhages after food had been taken. No cause could be assigned for its development. M. Méplain determined to treat it with repeated application of caustic, and for this purpose selected chromic acid, which he painted over its surface four days running. No improvement resulting, he next attempted to remove it with a pair of scissors curved on the flat: the hæmorrhage was moderate, and was easily arrested with a compress moistened with perchloride of iron. In three weeks the patient returned with the tumour, which had reached its original size. On this occasion he seized it with a pair of dressing forceps, and endeavoured to tear it away. Moderate bleeding only followed, and this was soon stopped with a little acid gargle. Eight days after the patient again presented himself, and M. Méplain, somewhat discouraged by his failures, recommended him to try frequent application of carbolic acid. This was found to be very difficult, troublesome, and painful, but it appeared to check the growth of the tumour. Once more it was removed with the scissors, and once more it returned in the course of a week. Galvano caustic he had not the means of applying, but he proposed the actual cautery, to which the



patient strongly objected. He therefore at length determined to inject with acetic acid. He only threw in one drop with an Anel's syringe, but the effect was most satisfactory. The pain was acute for a short time, but the tumour gradually wasted away and did not return. (*Bulletin Général de Théraputique*, t. lxxxv. p. 547.)

**Thrombosis of the Cerebral Veins and Sinuses of the Dura Mater.**—Dr. John Lidell, in a valuable paper on this subject, founded on the written histories of 128 cases, states that there is (1) a traumatic form of thrombosis of the sinuses of the dura mater. The blood sometimes coagulates in one or more of these sinuses directly in consequence of local injury. In such cases the traumatic lesion usually consists either of laceration of the walls of the affected sinus, or of laceration of some of the tributary veins, with contusion of the walls of the sinus, and is generally associated with injury of the skull in the form of compound comminuted fracture. (2) A not unfrequent cause of thrombosis of the cerebral veins is anthrax of the face. In a large proportion of these cases death rapidly supervenes, and thrombosis of the facial frontal or temporal vein, of the ophthalmic vein, and of the cavernous and other sinuses of the dura mater, constitute the connecting link between the local disorder on the one hand and the cerebral and pyæmic symptoms which follow it on the other. (3) In occasional instances ulceration of the mucous membrane of the nose is attended with erysipelas of the face, and gives rise to thrombosis and secondary phlebitis of the frontal and ophthalmic veins, and of the cavernous and circular sinuses. (4) Injury of the skull, when followed by caries of the bone and inflammation of the surrounding parts, not unfrequently induces thrombosis of the corresponding sinuses of the dura mater. (5) Suppurating wounds of the scalp, whether accompanied or unaccompanied with injury of the bone. (6) Suppurating diseases of the hairy scalp, such, for example, as eczema, sometimes produce thrombosis. (7) Purulent meningitis may lead to thrombosis of the veins of the pia mater, and of the superior longitudinal and other sinuses of the dura mater. Many cases are given in support of each of these propositions. (*American Journal of Medical Science*, January 1874.)

**Treatment of Diabetes.**—In a paper recently published, Dr. Kratschner gives the results of a series of observations he has made on a diabetic patient, with a view of testing the value of carbonate and sulphate of soda, and of morphia, upon the excretion of sugar. He finds that neither the carbonate nor the sulphate appeared to exert any influence on the amount of sugar excreted, but he has satisfied himself that in morphia we possess

an agent that is not only capable of materially reducing the excretion of sugar, but also of diminishing to a remarkable extent the general tissue metamorphosis of the body. (*Sitzungsberichte der Kais. Akad. d. Wiss. zu Wien.* Band lxvi. Heft iii. iv. and v.)

**Treatment of Tetanus.**—Dr. Gaston, of Sao Paulo, Brazil, gives the details of an interesting case of traumatic tetanus resulting in recovery. The patient was an able-bodied negro, who had received a blow upon the left side of the head, traversing the parieto-occipital suture, and about  $2\frac{1}{2}$  inches in length. He continued to work for several days. Dr. Gaston was called in on the sixth day after the accident. He had then slight opisthotonos and lockjaw. The wound was found in a state permitting it to be readily opened to the bone, but the bone was uninjured. The edges had a dark sanious appearance, without any indication of suppuration. A piece of lint saturated with spirits of turpentine was placed in the wound, and a poultice of corn-meal with flax-seed was applied over it. One grain of tartar emetic was then given every fifteen minutes for ninety minutes, and the patient was placed in a hot bath. On removal from the bath a marked improvement was observed, and the patient fell asleep: on awaking, spasmodic rigidity of the muscles recurred. A combination of calomel, gr. v; tartar emetic, gr. j; nitrate of potash, gr. x; and morphia, gr.  $\frac{1}{2}$ , was now given every two hours, until six hours had elapsed, and afterwards three table-spoonfuls of castor-oil with a table-spoonful of turpentine were administered, which procured evacuation, without, however, inducing free purgation. The spasms were now milder, and recurred with longer intervals, yet were still of a distinctly tetanic nature. Frictions with camphorated spirits of wine were made frequently throughout the entire extent of the spinal column and over the epigastric region; and a flannel moistened with this was kept constantly applied over the abdomen. In the mean time a blister was applied to the occiput and neck. At the end of the second day's treatment, one grain of morphine was administered at night, and repeated within four hours, and after the second dose the patient slept several hours; but the morning of the third day of the treatment, and the fourth of the disorder, found him still labouring under rigidity of the entire muscular system, with occasional attacks of opisthotonos and trismus, accompanied by spasmodic action of the diaphragm, that gave rise to a sound closely resembling hiccough. Regarding this stage of the disease as involving chiefly the nervous system, a wineglass of lac assafœtidæ was given every three hours, and an enema of half a pint of the infusion of tobacco, with a teaspoonful of laudanum, was ordered to be given and

repeated in six hours. The attacks became still more moderate, but the disease did not yield entirely. On the following day, which was the fifth day of continued tetanus, 10 grains of sulphate of quinine, with half a teacupful of infusion of valerian, was administered every three hours, until a drachm of quinine was taken. At the same time injections of lac assafœtidæ with laudanum were given every six hours. The sixth day did not find him free from spasmodic rigidity and recurrence of the convulsive attack; but considering the dangerous violence of the disease as having been subdued, he was ordered simply to use the infusion of valerian and an occasional injection of lac assafœtidæ with generous diet. On the eighth day, strychnia in doses of  $\frac{1}{16}$  grain was administered every two hours, but the tetanic rigidity being intensified, it was abandoned in twenty-four hours. Subsequent to this a combination of two grains of ipecacuanha, one grain of opium, and half a grain of calomel, was repeated every four hours during the day, till slight ptyalism supervened. A full dose of castor-oil now produced a most salutary effect, and the treatment from that time (twelfth day) was simply addressed to building up the exhausted frame of the patient, and without any recurrence of tetanic symptoms. (*Gaston Clinical Reports*, in the *New Orleans Med. and Surg. Journal*, vol. i. No. 2.)

## Notes and Queries.

### DEPARTMENT OF ANALYSIS AND INVENTIONS.

MESSRS. FELTOE AND SONS' SPÉCIALITÉ SHERRY.—The Rev. Sir Edward R. Jodrell, Bart., in his disinterested zeal for the public welfare, has had the above sherry analysed at his own expense, and has placed the report of the analyst at the disposal of Messrs. Feltoe and Sons, who make use of it as an advertisement. As this advertisement has found its way into several professional journals, we deem it a duty to our readers to point out the real nature of this much-vaunted sherry. Before doing so, however, we must express our regret at seeing Dr. Redwood's respected name attached to the analytical report in question; firstly, because we deem the use of such reports for mere purposes of trade exceedingly mischievous; secondly, because the report in question is full of errors, both of fact and of interpretation.

The total acid is given as amounting to 0·54 per cent. (we presume it is meant that the free acid present is equivalent to 0·54 per cent. of tartaric acid), and it is implied that this is about the usual amount of acid found in good samples of sherry, in addition to which it is alleged to be the *true acid of the grape*, namely, *tartaric acid*. Now, in the first place this acidity is rather high, for a young sherry, such as this evidently is; and in fact, assuming the acetic acid to have been estimated correctly, the acidity is, for a sherry, exceptionally high. In the second place, even in the grape-juice itself the greater part of the free acid is usually malic acid, the smaller portion only being tartaric acid. In all wines, even when perfectly natural, the proportion of tartaric acid is still further reduced by the precipitation of the tartar, owing to the alcohol produced by fermentation; whilst, in all sherries, the tartaric acid is reduced to a minimum, or is even removed entirely, by the plaster of Paris universally employed in Spain in the process of sherry-making. We are thus reduced to this alternative, either a serious error has been committed in the analysis, or this



reputed sherry is altogether a factitious article. Again, the proportion of ash is given as 0·45 per cent., and it is stated that this is not more than it should be, and that it contains *nothing foreign to the grape*. Now, if we assume this wine to have been plastered, like all sherries, this is certainly about an average amount of ash, but then it must contain a large proportion of sulphate of potassium, the greater part of the sulphuric acid of which must be derived from the plaster of Paris employed, and is therefore *foreign to the grape*. If, on the other hand, the wine has not been plastered, the proportion of ash is about twice as high as it should be in a natural wine, and we are again forced to the conclusion that we are dealing with a factitious article.

So much for this most elaborate analysis, as the Rev. Baronet terms it, ironically, as we cannot help thinking. Now for the actual facts of the case.

A sample of the Spécialité Sherry, procured from Messrs. Feltoe and Sons, 26, Conduit Street, yielded the following results:—

Specific gravity at 60° Fahr. . . . .	985·9	
Absolute alcohol, by volume . . . . .	22·45	per cent.
Equal to proof spirit . . . . .	39·5	”
Total free acid (calculated as tartaric acid)	0·44	”
Containing acetic acid . . . . .	0·15	”
” tartaric acid <sup>1</sup> . . . . .	0·03	”
Sugar and extract . . . . .	2·01	”
Ash . . . . .	0·445	”
Containing sulphate of potassium . . . . .	0·405	”

These results show that the wine in question is probably an ordinary sherry, tolerably strongly fortified, and of average acidity. Like all sherries, it contains scarcely a trace of tartaric acid, but very much sulphate of potassium, and has therefore unquestionably been subjected to the usual plastering.

## CORRESPONDENCE.

AMYL NITRITE.—Dr. W. H. Madden, of Torquay, sends us the following note:—

“Your notice of Dr. Pick’s work, in the current number of the *Practitioner*, induces me to send you this small contribution to amyl nitrite therapeutics.

“At the close of last year, during the illness of a colleague, I attended, for him, a lady of middle age, who was profoundly anæmiated by severe and intractable menorrhagia. The dis-

<sup>1</sup> The rest of the free acid is chiefly malic acid.



charge had almost ceased when I first saw her, but she was extremely feeble, and complained much of dyspnoea and utter inability to take food. She had been previously subject to chest troubles, chiefly, I believe, of a bronchial character, but there were no evidences of present mischief. I should mention that she was a large-built and very stout woman, and that the heart's action was feeble, though the sounds were healthy. In a few days the hands and feet became œdematous, the dyspnoea was intensified into orthopnoea, and auscultation showed that effusion was taking place rapidly into the bronchial tubes.

"On the morning of Dec. 21st the symptoms were exceedingly grave, and I asked for a consultation. Dr. Radclyffe Hall saw her with me, and confirmed my worst fears. A few hours afterwards I visited her again. I was detained down-stairs a short time, as she had insisted upon being taken out of bed, but I soon received a hasty summons. I found her apparently dead; the face and lips ashy grey; the jaw fallen; no pulse, no respiration; the pupils insensible to light, but the eyeball moved slightly when the finger touched the conjunctiva. While I was looking at her there was a shallow, gurgling gasp, and a lady friend, who was supporting her head, said, 'Should she smell anything?' I suddenly thought of the little bottle of amyl which I always carry in my pocket for my own possible necessities, and though I really believed the patient was dead, I took out the cork and held the phial to her nostrils. For a minute or more there was no result; then she drew a longer breath, and another yet longer; a faint tinge of colour made its appearance on lips and cheeks, and the pulse became slightly perceptible at the wrist. I continued the use of the amyl until respiration and circulation were fairly established, though she remained unconscious. On my next visit, after a short interval, I found her perfectly conscious, and more generally comfortable, though extremely ill. Dr. Hall and I met again in the evening. Matters continued much the same, the breathing very laborious, the air-tubes universally clogged, and the weakness great. She coughed frequently, but could not relieve herself by expectoration. During the night the amyl was used several times, when she appeared to be falling into the same condition as that which I witnessed, and always with relief; but in the early morning everything failed, and she died exhausted.

"I have not said anything about the general treatment adopted, as that is beside my present purpose; but I think the case shows, in a remarkable manner, the power which the amyl nitrite possesses to rouse a heart which has almost ceased to beat. Had the air-tubes been a little more free, I believe she would have rallied completely; as it was, her life was prolonged for fully eighteen hours.

SPIRITUS ETHERIS NITROSUS.—Mr. H. Maturin, Hartley Row, Hants, writes to us as follows :—

“In the last number of the *Practitioner* you give an analysis of Sp. Eth. Nit. B. P. purchased from a famous wholesale establishment, and it appears that the preparation was anything but the Sp. Eth. Nitros. of the Pharmacopœia.

“I believe you would be doing the profession a good service if you would procure (from some of the wholesale druggists that chiefly supply general practitioners) and make an analysis of some of the B. P. preparations that are in the habit of being most frequently prescribed, and after doing so give the results of the experiments in the *Practitioner* with the names of the druggists from whom the preparations were purchased, and the results of analysis. The practitioner in the country has no means of accurately verifying whether the drugs he purchases and prescribes are or are not such as they are represented to be, but has to accept everything for the most part in faith. The daily increasing desire for accurate therapeutical knowledge makes the question the more important.”

[We are glad to observe that the vulgar abuse which was heaped upon us in a leading article of our hitherto decently conducted contemporary, the *Pharmaceutical Journal*, on account of our remarks on this subject in the February *Practitioner*, has not prevented sensible men from heeding our words. A letter from a druggist, in the very next number of the *Pharmaceutical* to that which contained the explosion of editorial spite against us, simply and candidly took up a position which justifies our whole criticism. We are fortunately saved from any necessity of replying to the arguments of the *Pharmaceutical Journal* by the remarkable *exposé* which its editor gave of his own degree of qualification as a pharmaceutical critic. He supposes—it is impossible to imagine why—that officinal sp. eth. nitrosus should contain 10 per cent. of real nitrous ether. The fact is that it must contain about 3 per cent., in order that 2 per cent. may be separable by the Pharmacopœial test. After that, it does not seem worth while to trouble our readers any further about the editorial opinions of our contemporary.—ED. *Pract.*]

BROMIDE OF SODIUM.—Dr. Ainslie Hollis sends us the following note on Prof. Binz's paper “On the therapeutic employment of bromide of potassium.”<sup>1</sup>

“I cannot suppose that even Prof. Binz's zeal for a beloved theory would lead him to publish a voluntary misstatement of fact, and I shall ascribe the following errors in his quotations from my paper to the Professor's ignorance of our language. The passages, however, as they stand in his paper might convey

<sup>1</sup> *Practitioner*, January 1874.

a false impression to your readers, and as their removal to a great extent destroys the appositeness of his sarcasm, I trust you will find room for their correction. They are as follows (page 14):—

“1. After stating that I had taken 22 (I wrote 25) grains of the salt at bedtime, he says: ‘On the following morning there were *deafness* and tingling in both fore-arms and in the right leg.’ My own words were ‘*numbness* and tingling,’ &c. The word ‘*taubheit*’ in the original paper is badly selected, as it is liable to this misconstruction. My auditory nerves were unaffected by the dose.

“2. With regard to my first case he remarks: ‘After a long course of Na Br, the attacks became milder. But the patient was taking succus conii at the same time.’ If Prof. Binz will re-peruse my paper, he will find that the succus conii in small doses was added to a draught of bromide of potassium, which the man took previously to the trial of the sodium salt, and with doubtful advantage. The sodium salt was given in water only, as stated towards the end of my paper. I do not think that Dr. John Harley or any other gentleman would admit that the succus conii in a small dose would hold its sway over the system for three weeks after its administration.

“3. In the same case he writes: ‘The patient had a recent seton at the nape of the neck.’ Now, it will be found that upwards of two months elapsed after the insertion of the seton before I commenced treating the patient with sodic bromide. At that time the seton was nearly healed, and any therapeutic value attached to its operation had already been observed.

“4. The third case he calls the ‘cure of a man, aged 25, who at the time was having five fits daily, by the *long-continued* administration of 15 grains three times a day.’ My own words are: ‘A cigar-maker, aged 25, who had occasionally as many as five fits in one day, had no return of the fits after taking daily 15 grains of the sodium salt in three doses of 5 grains each.’ This patient was relieved of his fits *immediately* after the commencement of this treatment; the fits on two occasions were as many as stated, but usually occurred at the average number of four or five weekly. I do not pretend to *cure* patients of epilepsy, and expect to ward off the fits only whilst the treatment I have adopted is pursued. Finally, as shown by the context, 15 grains were taken daily, and not three times daily. I have, since writing the above-mentioned paper, tried the sodium salt upon upwards of a dozen fresh cases, and I still adhere to my previous opinion as to its value as a nervous sedative in cases of epilepsy. It appears, as does also the potassium salt, to lose its efficacy in checking the seizures after long-continued treatment, unless it is given in increasing doses.

In persistent cases I think it will be found useful to alternate this remedy with the potassium salt in their treatment.

"If Prof. Binz (to paraphrase his own words) wishes anyone to be enthusiastic about potassium in the treatment of epilepsy, he must be more careful, in criticising the statements of others, to ascertain first whether he accurately interprets their meaning.

"Yours obediently,

"W. AINSLIE HOLLIS."

[We acknowledge, with due penitence, the hasty mistranslation of the word *taubheit* as "deafness" instead of "numbness:" not a very criminal error, as the word really has both those meanings. Also, the figure 22, instead of 25, is *our* mistake, not Prof. Binz's. By an unfortunate chance the number of the *Practitioner* which contained the original paper of Dr. Hollis was not at hand at the time, or we should have escaped these errors.—ED. *Pract.*]

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Applications de l'Électricité à la Médecine et à la Chirurgie. Par le Dr. A. Tripier. 2<sup>me</sup> edit. Baillière. 2f.

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<sup>1</sup> Any of the foreign works may be procured on application to Messrs. Dulau, of Soho Square, W.C.; Williams and Norgate, of Henrietta Street, Covent Garden, W.C.; or Baillière, of King William Street, Charing Cross.



## Department of Public Health.

### THE COLLAPSE OF THE SANITARY ORGANISATIONS UNDER THE PUBLIC HEALTH ACT 1872.

IT is not difficult to foresee the rapidly approaching collapse of the sanitary organisations which have been formed under the Public Health Act 1872. However wishful the new President of the Local Government Board, Mr. Selater-Booth, may be to remedy some of the more glaring anomalies brought about by his predecessor, it must quickly become apparent to him that the only hope of escape from Mr. Stansfeld's amazing muddle is in the provisional nature of the appointments made under the Act. The Local Government Board will eventually have to recommence the work of sanitary organisation upon some more coherent principles than those hitherto adopted, but in addition to the ordinary inertia of local authorities it will now have to contend with the great, justifiable, and increasing distrust which its recent doings have given rise to among them. It would be well, indeed, for the reputation of Central Sanitary Administration, and, which is of still more importance, for the kingdom at large, if the sanitary work of the Local Government Board since the passing of the Public Health Act 1872 could be swept away. Indeed, so grave are the mischiefs threatened, or rather occasioned, by the wrong direction given to the present time by the Board to the efforts of the new sanitary authorities created by the Act, that it has already become a matter for consideration whether the whole question of Sanitary Administration, presumed to have been settled for a while at

least by the Royal Sanitary Commission of 1866, should not be again opened. Mr. Rumsey, at least, is of this opinion, and his opinion will probably influence very considerably the action of the British Medical Association. Whether the present Government would be prepared to deal thus thoroughly with the question at an early period of its existence it is difficult to surmise; but the presence of the Chairman of the late Sanitary Commission among the Ministers does not give much hope that such would be the case. It could hardly turn a deaf ear, however, to requests for an inquiry into the measures which have been adopted for the initiation of the Public Health Act 1872, and the results which have flowed from them, as preliminary to further legislation. Such an inquiry would bring out, we believe, in the strongest light the executive shortcomings of the Royal Sanitary Commission's recommendations, and of the measures, both legislative and administrative, which have been founded upon them. The *Times* has suggested this course, and it is one which commends itself from its practical, and it may be said, perhaps, practicable nature.

The account we gave, in the last number of the *Practitioner*, of the inconsistencies and incoherences of sanitary organisation which Mr. Stansfeld has left as a legacy to his successor, would alone prove the necessity which exists for an early inquiry of the nature here indicated. The facts there detailed show either that the Local Government Board has been giving different and often inconsistent advice to the same end in different places, or that the former Poor-law (now the general) Inspectors of the Board have been permitted to advise sanitary authorities on the complicated issues of sanitary organisation and works without any common instructions, and each according to his own light. In the former case the Board has obviously acted without reference to previous experience on the subjects concerning which it has advised or to the special information it has at its command; in the latter case this line of action has been deputed to each of the several general inspectors of the Board, and the complications and difficulties connected therewith have been multiplied in proportion to their number. Whichever the correct interpretation of the action of the Board, the effect so far as local government is concerned is the same. Mr. Stansfeld

elected that the *direction*<sup>1</sup> which it was the peculiar function of the Central Government to afford to the newly created sanitary authorities should not take its starting-point from a well-digested accumulated experience of the duties imposed upon them, but should begin at the same level as their own knowledge. The general inspectors of the Local Government Board were, in respect to a knowledge of sanitary organisation and work, in no better position than the sanitary authorities they were called upon to advise; and their previous duties had tended to localise rather than to generalise such common vague conceptions of sanitary organisation and requirements as most educated men have at this day. Assuming that any definite principle at all, other than that of maintaining the peculiar departmental arrangements and traditions of the old Poor-law Board, existed beneath Mr. Stansfeld's policy of sanitary administration, it was a principle of confounding the duties of central administration with those of local administration. Mr. Stansfeld dealt with the matter as if he were simply the chairman of a bigger Sanitary Board than any other Sanitary Board in the kingdom; and his inspectors, each in his own district, acted on this narrow conception of function. Local government was not to be put to shame by any assumption of larger knowledge by the Central Government. The agents of the latter were to confer with the agents of the former,<sup>2</sup> and from the combined ignorance of the subject matter of conference, wisdom was to be educed. But, alas! the local mind was quite unequal to the conception of such astute policy (in view of a general election). Either it looked upon the general inspector of the Board as a species of apotheosis of the local government mind, and so deferred to him without question as an inspired agent, or it took a common-sense, business-like view of the subject, and looked to the agents of the Central Government for information for its guidance as to the intentions of the Legislature in respect to the new Public Health law, and to the duties imposed by it upon local sanitary authorities. In both

<sup>1</sup> "The Central Authority must avoid taking to itself the actual work of local government. . . . We would leave *direction* only in the Central Power."  
—*Report R.S.C.*, p. 35.

<sup>2</sup> See Mr. Stansfeld's answer to a Medical Deputation, *Times*, June 22, 1873.

cases the result was the same. Human nature (even official human nature) is weak. The guarded conferences in which Mr. Stansfeld proposed to throw all the responsibilities of inception and suggestion upon the local authorities, resolved themselves into meetings in which the Board's general and assistant inspectors took the initiative and laid down those marvellous different schemes of sanitary organisation which unintentionally repeat the vagaries of the College of Laputa. These for the most part were acted upon without question by the sanitary authorities to whom they were submitted, and their outcome was briefly shown in our last number, and should form a subject of inquiry by the new Government before it commits itself to sanitary legislation. The responsibility for this outcome rests absolutely with Mr. Stansfeld's administration at the Local Government Board, an administration as inconsistent with the principles of true local government as it was with the principles of true central government. It was an administration of "meddling and muddling" which has led to consequences that have thrown back the local sanitary work of this country for several years, and has created additional local difficulties in the way of effective local sanitary organisation which will be hard to overcome.

Already we are beginning to see the end of the sanitary organisations already effected under the advice of the Local Government Board. From various sources information is coming in upon us to the effect, on the one hand, of the extreme discontent of some sanitary authorities with the arrangements they have carried out consistently with the advice of the Board's general inspectors, and, on the other hand, of the extreme gratification of other sanitary authorities with the futility of these arrangements. The authorities (not a few) who have some regard to the new duties imposed upon them resent the position in which they have been placed by appointing officers under conditions which make a proper performance of their duties impossible. The authorities (also not a few) who have no desire to bestir themselves in sanitary work, rejoice that by following strictly the advice of the Local Government Board they have fulfilled the letter of the law and placed themselves in a position in which any serious attempt at sanitary work is unne-



cessary because it would be futile. One correspondent writes to us :—

“I live in a rural district of some eighty square miles extent, and having a population of about 70,000. Twelve months ago, acting under the strongly urged advice of the inspector of the Local Government Board, and reposing trust in his superior wisdom, we appointed our Poor-law medical officers Medical Officers of Health, giving them a small increase of salary for the additional duty. At the same time, acting on the same advice, we appointed a solitary Inspector of Nuisances for the whole district, at a salary of 150*l*. The result of our twelve months’ experience of this arrangement has been that the Medical Officers of Health have found it impracticable to carry out the duties of that office in any reasonable sense of the Regulations of the Local Government Board as to such duties; and that our Inspector of Nuisances has not been acting as such in the ordinary sense of the term, but has been, from the mere force of circumstances, performing the functions of an amateur Medical Officer of Health. Now our sanitary authority is a body which does not want a few shrewd men in it; and these men are now talking very freely of this state of things. They object to paying for officers who do not and cannot carry out the duties required of them, and they have come to the conclusion that this unsatisfactory and anomalous condition of things must cease. The inspector of the Local Government Board has suggested that while retaining the present arrangement, they should, in addition, combine with neighbouring sanitary districts, similarly placed as to their officers, to elect a highly-paid superior or superintending officer of health. This they flatly refuse to do, for the following reasons among others :—The advice first tendered to them was presented as all-sufficient for the sanitary wants of the district, the advice having the advantages of economy and (as it was said) efficiency, and no suspicion of the need of such a co-ordinating element as that now suggested existed. This last element may, they think, be required to obviate the ill results of blundering advice of the inspector, but there is no evidence that it would conduce to the desired efficiency, while it would certainly cause an unexpected and unwelcome addition to the cost of organisation. Now what are



we to do to get out of our mess? Strange though it may seem, we are really wishful here to go soberly and soundly to work to amend our sanitary state. With these specimens of central advice before us, and the melancholy results of acting upon the first-given specimen, we are much disheartened and don't well know how to turn. We have been virtually instructed by the Local Government Board to set aside all prepossessions as to the duties and capabilities of Medical Officers of Health and Inspectors of Nuisances as derived from experience, but a committee (which I have consented to join) will, even at the risk of offending the powers that be, presently start on a little tour of discovery. Of course we shall avoid official lights as we should avoid—well, let us say, a nuisance. No doubt, the medical lights of the Local Government Board deserve all the good things that you have said of them, but it is not for us to question the wisdom of the Board in putting them aside in cases such as ours. Moreover, after having been scorched by the particular official light which the Board has bestowed upon us, we are not disposed voluntarily to subject ourselves to the chance of another scorching. No, we prefer in a quiet business-like way to hunt up some of the experienced Medical Officers of Health in the metropolis and the provinces, and have a talk with them. We shall take the metropolis last, and if you don't object, we'll include you, for the nonce, among the fraternity. Dine with us on the Thursday of Easter week at the 'Ship,' and be prepared, like a good fellow, to unmuzzle your wisdom (and eke your tuneful voice) over a bottle of the marvellous Heidsick there."

From several correspondents residing in combined sanitary districts we have information that the question of determining the appointments of the Medical Officers of Health, at the end of the period to which the appointment is in the first instance limited, is beginning to be seriously discussed. The obvious failure of these appointments, in many cases, to meet the requirements of office as laid down by the Local Government Board, is the ostensible reason alleged for this course.

It is painful to have to contemplate the frustration of the Public Health Act 1872, in the above-mentioned respects, and the disappointment and dissatisfaction which must be largely

experienced among the profession by the lapse of the organisations already made under it. The occasion is one in which the profession, for its own sake as well as for the public weal, should take energetic action, to impress upon the new President of the Local Government Board the necessity of imparting some different counsels from those which guided his predecessor in his administration of the Sanitary Laws. Meanwhile the ulterior views advanced by Mr. Rumsey should not be lost sight of; and while due care is being given to the present requirements, no effort should be spared to bring about those wider and more radical changes of the Sanitary Laws which will permit a rectification of sanitary areas and a more rigid control over sanitary organisations.

#### PORT SANITARY OFFICERS.—THEIR QUALIFICATIONS AND DUTIES.

UNDER the Public Health Act 1872, Sec. 20, the Local Government Board is empowered to constitute port sanitary authorities, and to assign to them such powers and rights under the Sanitary Acts as may enable them to fulfil their duties. Forty-seven port sanitary authorities would appear to have been constituted by the Board to the present time. The port of London was dealt with, and the sanitary authority for it, the Corporation, was designated by the Act itself. Following its customary reticence in such matters, the Local Government Board issued no instructions for the guidance of the officers appointed by port sanitary authorities, leaving them and their authorities to apply the regulations of the Board's orders as to Medical Officers of Health and to Inspectors of Nuisances, in such way as they might think fit. Dr. Harry Leach, the Medical Officer of Health for the port of London, has here come to the assistance of his brother officers, and in a recently published report to his Port Sanitary Committee he has offered a series of suggestions for their guidance. These suggestions are well-timed, and they carry the weight of a long and exceptional experience of ships and sailors. Dr. Leach's intimate acquaintance with the Thames below London Bridge and long connection with the "Dreadnought" hospital has given him

a rare knowledge of the sanitary requirements of shipping, and his present appointment has now been held for a sufficient length of time to enable him to write with authority on the exercise of the duties of port sanitary officers.

The report to which we have referred, in addition to the subject of this notice, is of interest as giving the first systematic account of the sanitary requirements of the port of London. This port extends from Teddington Lock to the North Foreland, and is not less than 88 miles in length. It includes eight sets of docks and thirteen creeks, and is surrounded by no less than forty-six water-side authorities. The principal portion of the sanitary work of the port is confined to that part of the district which lies between London Bridge and Woolwich Arsenal Pier, a distance of ten miles. "In this section are moored," writes Dr. Leach, "on both sides of the river, a constant average of no less than 400 vessels of all descriptions, more than 90 per cent. of which have crews on board. A large number of these vessels require constant general inspection, with reference to closets and forecastles, for the work of clearing and filling up with cargo is carried on at so rapid a rate that sanitary measures are in many cases constantly neglected unless constant reminders are given. Many of the old coasters . . . are in a bad sanitary condition, and require supervision the more as they do not come under the cognisance of the officers of Customs." The dock space in the port amounts to 488 acres, seven sets of docks being on the north and one on the south side of the Thames. The total average number of vessels lying in these docks is from 600 to 700, and of these 50 per cent. have crews on board. Dr. Leach estimates that under ordinary circumstances an inspector may visit 60 or 70 vessels a day; and he is of opinion that a staff of three inspectors would be required to do the regular sanitary work of the port. He observes, of vessels in dock, that "the crews living on board are placed in very unfavourable sanitary conditions. The ventilation of deck-houses and forecastles is frequently insufficient, the crews are often idle, and therefore more dirty than usual; a great deal of carelessness sometimes exists as to the water-supply; and, chiefest of all, these men are floating, as it were, on cesspools of their own making, inasmuch as the contents of all latrines and

closets must of course flow into the docks." Dr. Leach has taken measures to remedy this serious evil by inducing ship-owners to provide for the accommodation of the crews on shore. The hospital accommodation of the port consists at present of an old hulk, lent by the Admiralty, and moored off Gravesend. Dr. Leach recommends the establishment of two small floating hospitals, to be moored, one off Gravesend, the other somewhere between London Bridge and Woolwich, say off Deptford, and each to accommodate from sixteen to twenty patients. In an appendix, Dr. Leach gives instructive diagrams of the arrangements of the floating hospitals established on the Tyne, at Dublin, at King's Lynn, and at Lowestoft.

As Dr. Leach's report is not generally accessible, we reproduce his suggestions as to the qualifications and duties of Port Sanitary Officers :—

*Qualifications of Port Medical Officer.*

The information to be acquired by a Port Medical Officer of Health should comprise—

A thorough knowledge of the geography of the port in which he holds office, including its docks and creeks, and all outfalls, whether of sewage or otherwise, to high-water mark.

A knowledge of the general construction of the hulls of wooden and iron vessels from upper deck to keel, of the general arrangement of the crew spaces, modes of berthing, amount and quality of ventilation, arrangement of latrines, bulk-heads, ports, scuttles, up-casts and down-casts, state of bilges, &c.

Those clauses of the Merchant Shipping Acts, and specially those of the Merchant Shipping Act of 1867, that relate to health of crews.

The sanitary clauses of the Emigration Acts.

The Public Health Act of 1872 and the Acts therein included, so far as they relate to Port Sanitary Authorities.

The scales of diet used commonly in coasters, ocean steamships, and sailing vessels respectively.

The average number and nationalities of vessels frequenting the port, and the sort of cargoes that they bring.

The kinds and varieties of water-tanks commonly used on board ship.

The sources of water-supply for inward and outward-bound vessels, how delivered on board, and, if delivered from tank-boats, the condition of the tanks in those boats.

The average proportion of vessels lying in the jurisdiction that have crews living on board.

A full and continuous knowledge of the weekly and quarterly bills of mortality issued by the Registrar-General of Births, Deaths, and Marriages, and of the causes of deaths of sailors who die at sea.

Early information as to the presence of any special epidemic (as small-pox) in any of our own outports, or in any ports in the basins of the Mediterranean or Baltic Seas.



*Duties of Port Medical Officer.*

To pay special attention to all vessels, particularly coasting vessels, the crews of which live on board.

To indicate daily, or as may be required, to the Sanitary Inspector, such vessels, in order that they may be systematically visited and examined.

To inquire into the water-supply of all vessels in the port, and advise as to its proper sources and stowage.

To superintend the immediate removal from a vessel of any person suffering from any contagious or infectious disease, to the hospital set apart for the purpose by the Sanitary Authority, or if the sick person is not in a condition to be removed, to isolate the vessel. See 29th and 30th Vict., cap. 90, sec. 29.

To superintend the disinfection of all clothing of seamen who have died from any contagious or infectious disease, and to grant a certificate accordingly.

To inspect, before landing, all emigrants that arrive in the port from the Continent for purposes of transhipment, and to isolate all suspected cases.

To carry out, under the direction of the Port Sanitary Authority, all special Orders in Council relating to the prevention of cholera, or other epidemic diseases.

To obtain all possible information as to, and keep a close account of, all foreign ports infected with, or suspected of, cholera, and with the aid of the Customs Officers, to inspect all vessels as they arrive from such ports.

To report to the Marine Department of the Board of Trade, without delay, any defect in cubic space for the accommodation of the crew, the existence of scurvy on board ship, or any breaches of those clauses of the Merchant Shipping Act of 1867 that relate to health of crews.

To advise shipowners, with the sanction of the Sanitary Authority, as to any defective sanitary arrangements in their respective vessels, and to consult with them, if requested, as to remedying the same.

To be ready at all times to advise ship-masters as to sanitary arrangements afloat, and specially with regard to the water-supply, the state of the bilges, &c.

To examine and keep a record of all reports respecting examination of vessels handed in by the Sanitary Inspector, to classify and summarise the same, and to present them to the Sanitary Authority at each and every meeting.

To attend all meetings of the Sanitary Authority, and present a synopsis of the work performed since the previous meeting.

To submit to the Sanitary Authority a yearly or half-yearly report containing a detailed account of all duties performed by the officers.

To examine (if requested to do so by the crew of, or any other person living on board, any ship or vessel) any article of food that is declared to be unfit for human consumption, in order that, if necessary, the order of a justice of the peace may be obtained for the destruction of such article. See 26th and 27th Vict., cap. 117, sec. 26.

To obtain the sanction of the Sanitary Authority under whom he acts before initiating any new line of action, or attempting to carry out any special sanitary reforms not hitherto attempted.

To communicate and co-operate in all sanitary matters with officers of her Majesty's Customs, the Marine Department of the Board of Trade, the harbour and dock authorities, the river police, and all other authorities concerned in the official business of the port.



*Qualifications of Port Sanitary Inspector.*

1. A general knowledge of ships and seamen, to which end it is desirable that he should have had some service afloat.
2. A general knowledge of the scales of diet in common use in coasting and ocean-going ships.
3. A good geographical knowledge of the port to which he is officially attached.
4. A conciliatory but decisive mode of conducting inspections, so that the abatement of nuisances or any other sanitary work required to be done by the owners or masters of the vessels may be performed without any necessity for serving a notice or applying for a summons.

*Duties of Port Sanitary Inspector.*

1. To act generally, with the approval of the Sanitary Authority, under the directions of the Medical Officer of Health.
2. To inspect and otherwise examine into the sanitary condition of all vessels within the jurisdiction of the Port Sanitary Authority, in accordance with directions given daily, or from time to time, by the Medical Officer.
3. In pursuance of this duty to see the officer in charge of each and every vessel inspected, and obtain from him the following particulars:—(a) Name of vessel; (b) nationality; (c) sailing, steam, or barge; (d) where from; (e) number of crew; (f) cargo.
4. To examine the closets, heads, and latrines.
5. To examine the quarters of the crews with reference to cubic space, cleanliness and ventilation.
6. To note the existence of any contagious or infectious disease, and to report such cases immediately to the Medical Officer.
7. To advise the officer in charge (if there be no Medical Officer attached to the ship) to send all other cases of sickness to the nearest hospital, if they can be removed without danger to life.
8. To note the existence of any foul cargoes, and to record their descriptions.
9. To call the attention of the Medical Officer to all sanitary defects observed during each and every inspection.
10. To direct the cleansing of all closets, forecastles, &c., in which sanitary defects exist, and to revisit such vessels to see if the directions have been carried into effect.
11. To fumigate and disinfect vessels and clothing as required, and in the manner prescribed by, the Medical Officer.
12. To serve "Notices" only with the sanction of the Sanitary Authority or the Medical Officer.
13. To write out at the end of each and every working day, in an official book of forms prepared for the purpose, an account of duties performed, and to submit the same to the Medical Officer for examination, in order that these reports may be laid before the Sanitary Authority at every meeting.

## THE VENTILATION OF SCHOOLS.

OUR Transatlantic contemporary, the *Sanitarian*, has recently published several instructive articles on "School-house Ventilation." Reading them, and studying the plates with which they are illustrated, the mind is carried back to the early days of sanitary work in this country, when commissions, committees, enthusiastic inquirers, and ingenious inventors, revelled in wondrous diagrams. Who does not call to mind the pictorial sections of public halls, schools, churches, and dwelling-houses then in vogue, in which the distribution of spoiled air was represented by graduated blue tints, which tints have now become almost inseparably connected with our notions of a defiled atmosphere? Then there were the bewildering diagrams of the movements of air in rooms under certain conditions, the direction of the currents being shown by innumerable arrows, launched as it were against "the light militia of the lower sky." It would seem that this tendency to graphic illustration of matters connected with ventilation, and the tendency of mind of which it is the visible sign, still hold their own in the United States.

We presume there must be some climatic peculiarities in the United States which render it necessary that the question of the ventilation of schools there should be mainly a question of artificial means. At any rate, while here the whole tendency has been for several years to trust chiefly to "natural ventilation," so called, all the recent papers that we have received from the United States on school ventilation have shown a preference for systems of artificial ventilation, or dealt with the subject from this point of view as a determined necessity. And it must be confessed that the field thus opened to the ingenuity of inventors has not been idly cultivated. Reading the accounts of the arrangements for ventilation and warming which obtain in some of the schools of Philadelphia and New York, it is a marvel to us what the influence must be upon the physical and mental condition of the children, of the artificial atmospheres prepared for them. The subject, from this point of

view, is one which might well be commended to the notice of Oliver Wendell Holmes. We should like to have from him a "physico-psychological study," as an advanced French novelist would phrase it, of a young girl (Elsie Zephyr?) brought up under the conditions which appear to exist in these schools. If the different appliances serve their end, the occupants of the class-rooms and collecting rooms must during their occupancy be much in the same condition as delicate morsels in a Norwegian cooking-box. Happily school-hours are limited, and we apprehend that in the United States, as here, human appliances are subject to human fallibility.

What has chiefly contributed to the discredit of artificial systems of ventilation and warming in this country has been the impossibility, in the majority of cases, of obtaining for ordinary purposes that amount of attention to them which was necessary to secure their efficient operation; and in such cases it has been found that ventilation has had to be obtained (if obtained at all) in spite of the system rather than in consequence of it. An apt illustration of this fundamental drawback to artificial systems of ventilation is furnished in the *Sanitarian* for August last. In an article on "School-poisoning in New York" there is a description of the warming and ventilating arrangements of the City School House, which is peculiarly instructive. The class-rooms in this building are arranged to be warmed by furnaces in the basement. The fresh air is intended to be admitted to chambers surrounding the furnaces, and after being warmed there it is passed to the different classes. The inlet and the outlet openings of the class-rooms (both improperly placed) appear, according to the appended diagram, to communicate with the same flue. If this be the case, the upper class-rooms (the building having three stories) would receive, in addition to the warmed fresh air, foul air from the lower class-rooms. Now, on a late examination of these arrangements, it was discovered that the *fresh-air inlet* of one furnace had been *converted into a hencoop*, that one only of the other fresh-air inlets took its air from the external atmosphere and admitted a good supply, and that but one of the evaporating-pans attached to the furnaces had a supply of water, the others being dry and dusty. Moreover, on the reporter

ascending to the roof to examine the outlets of the foul-air flues, which were brought together in two louvred cupolas, he discovered that one of these cupolas had been *boarded up for a pigeon-house!* Our own experiences of artificial ventilation of buildings have been much to the same effect as in the above story. They began with a new hospital, built at a time when notions as to artificial ventilation were rampant in this country. The wards were all arranged to be ventilated by a "vacuum system." Shafts communicated with lofty turrets, in which big furnaces were placed. With these shafts were connected other shafts opening on the floors of the different wards. Inlets for fresh air were placed at the upper part of each ward communicating directly with the outer air. The experimental trials of this apparatus were a wondrous success. When the ward doors were closed, the registers of the open fireplaces shut down, the fresh-air inlets opened, and the furnaces of the exhausting-towers lighted, the air rushed up the shafts from the wards with the force of a small gale; and delighted committee-men exhibited to delighted committee-women and visitors the spectacle of strips of paper whipped out of sight by the ascending current and along the shafts, and rejoiced to think that infection and infectious matters could be got rid of with a like facility. When we made acquaintance with the hospital it had only counted a few months' existence; but we lived in it some little time before we came to a knowledge of its wondrous system of ventilation. The only officer who had known the building thoroughly was dead. The ventilating turrets were locked up, and the keys misplaced. The "outlet" openings in the floors of the different wards had been explained to us as *inlet* openings for warm air, but disused because the physicians preferred open fires. Diminutive ward windows, the sashes not reaching within four feet or more of the ceiling, were regarded as blunders, in which efficiency had been sacrificed to architectural effect—these windows having in fact been designed with reference to light alone, and not to ventilation. We lived in the hospital some months, and during that time the system of ventilation was never used, and we were never able to assure ourselves that it had ever been used after patients had been admitted to the hospital. "Sure, sir," exclaimed the senior nurse, "how could we use it, unless we screwed the beds to the



floor, and tied the patients to the beds?" At any rate the system was never used in our time, and, what is more, its want was never felt. For notwithstanding small window space, we contrived with this and the fresh-air inlets, and the doors and open fireplaces, to get all the ventilation that was desirable, and, much to the content of the home-committee, to spend on extra beds and patients the considerable cost that would have had to be devoted to the service and feeding of the furnaces, had their use, and the system of flues to which they were attached, been other than an ingenious sham.

With the above-stated experiences before us, and others to the same effect, in reserve we turn, with more admiration of its ingenuity than confidence in its practical working, to the plan for lighting, warming, and ventilating schools by Mr. Lewis W. Leeds, of New York, to which was awarded the premium at the late Vienna Exhibition. The drawings represent what Mr. Leeds describes as a "Sunned and Aired School-house" (the *Sanitarian*, December 1873). The plan is restricted to classrooms without a common room, and each room is so placed as to be disconnected from the others, and to receive a full proportion of sunlight. The principle of warming and ventilation Mr. Leeds seeks to carry into effect is thus stated:—"Nature's great means of purifying the air and supplying it fresh to all living things is AGITATION. The gentle agitation of the air in the schoolroom is of the first importance. This is very naturally and beautifully accomplished by having the floors warmed to 80 or 90°F., and the air above it only 50 or 60°F. This would set the whole air of the room in motion, similar to the water in a pot over the fire." How he proposes to accomplish these ends is shown in two charming diagrams, to which we would refer all interested in the subject, and in which the heat radiates, and the air ascends and descends, and flows in and out just as the delicate arrows show it ought to do, and would do, but for the combined perversity of heat, air, and human nature. Mr. Lewis W. Leeds is, however, one of the few designers who commands our regard and attention in this respect; for he is aware of the obstacles, and gives this caution, that "it is exceedingly difficult to make any plan that shall so cause a constant and uniform motion of the air at all times."



Dr. Ross, the Medical Officer of Health for the St. Giles' district of the metropolis, has recently discussed this subject from what we may term the American point of view.<sup>1</sup> What he has written upon it deserves an attentive perusal. He comes to the conclusion that the proper ventilation of a schoolroom cannot be secured except by artificial arrangements, and he lays down the principle that "the poisoned air should be carried out of the room as soon as it is breathed." To do this he would have extracting-flues placed on the sides of the room within a few feet of the floor, fresh air being admitted from the level of or near to the ceiling. He gives a drawing of one mode in which his notions might be carried into effect, and offers suggestions for cooling or warming, as the case may be, the incoming fresh air, and even for baking, so as to destroy infectious matters. We disbelieve in the possibility of any such rapid removal of the air fouled by breathing, as Dr. Ross suggests, to say nothing of the mechanical difficulties of regulating for any certain practical use such a nicely adjusted scheme of exhaustion of foul air and supply of fresh air as he recommends. A converse scheme to that of Dr. Ross's was suggested several years ago, in New York, but whether it was ever carried into operation we do not know. In this scheme the proposition was, if we mistake not, to dilute the fouled air as it issued from the mouth. To this end it was suggested either that the seats of the children should have fixed hollow backs, perforated in the rear, so as to deliver finely divided streams of fresh air above the desks behind at the level of the child's mouth; or that each child's desk should have a finely perforated air-chamber upon it fed from the external air.

It is generally forgotten that the difficulty of successfully carrying into ordinary practice schemes of this kind is, as a rule, in proportion to their ingenuity. If the same amount of trouble were bestowed in teaching school-teachers how to deal, for purposes of ventilation, with the simple arrangements of open fire-places and common sash-windows, as is given to inventing fanciful methods of warming and ventilation, the objections which attach to those commonplace methods would be dimi-

<sup>1</sup> "On the Ventilation of Schools, Hospitals, Law Courts, and other Public Buildings." Collingridge. 8vo. p. 29.

nished, at least in this country, to a vanishing point. It would be an easier task to teach school-teachers what it is practicable for them to understand, and what must of necessity be under their supervision, than to teach them complicated and perhaps doubtful systems of warming and ventilation, of which the supervision must be under some less educated person, and which cannot be under their immediate control. The London School Board very wisely contents itself, in its "*Rules to be observed in Planning and Fitting up Schools considered in reference to Schools of large size to be erected in London,*" to limiting the number of scholars in the school-rooms, and requiring a certain superficial area for each child, with "through ventilation." In practice this has had the excellent result of causing to be applied to school purposes the principles of warming and ventilation which have now been accepted in this country for hospitals. We shall presently return to this subject and describe some of the new schools erected under the School-Board Rules.

## THE INTERNATIONAL SANITARY CONFERENCE.

In the last number of the *Practitioner* we mentioned the probable occurrence of an International Sanitary Conference, on Quarantine, during the present year. We are now in a position (thanks to the Russian journal the *Messenger Officiel*) to state the circumstances under which this Conference has been arranged. What follows is a free translation from the journal above referred to. It will be observed that the approximation to English views of the opinions entertained by Russia as to quarantine, and the importance of the resolutions passed by the International Medical Conference held at Vienna last year on this subject (and quoted in our last number) as tending to a like approximation, are even greater than we believed. It is to be observed also, *à propos* of remarks made in our last number restricting the use of the term "quarantine," that the quarantine on land-routes referred to in the article is strictly analogous in character to the quarantine on sea-routes.

The International Sanitary Conference which met at Constantinople in February, 1866, had for object the preservation of Europe from invasions of cholera coming from the East. This Conference came to certain conclusions for the guidance of the different Governments interested in its work, which were based upon data derived from the facts of the pandemic extensions of cholera beginning in 1830 and in 1847. Cholera had again invaded Europe in 1865, and in the summer of 1866 the disease assumed the proportions of a terrible epidemic. From that time to the present, cholera, sometimes becoming more, sometimes less active, has never entirely died out in parts of Europe. The observations made during this period have enriched science with much new matter, and have modified the opinions entertained on the restrictive measures to be adopted against the epidemic, as was shown by the resolutions of the International Conference held at Vienna last year. This Conference voted by a large majority the suppression of quarantines on land and river routes, and held the maintenance of maritime quarantine to be necessary within certain limits only. A sanitary commission called together at Tiflis in 1870, under the presidency of Dr. Pelikan, the Director of the Russian Medical Department, formulated similar conclusions.

The quarantines which have been established against cholera on land and river routes have disturbed commerce without retarding the propagation of the disease, as was proved last summer in the north of Germany and on the Vistula.

Maritime quarantine is generally powerless to arrest the march of cholera, but the source of this powerlessness rests probably in the defective organisation of the quarantine establishments. As a means of observation and disinfection maritime quarantine has rendered service, and has at least retarded the propagation of the pestilence, and thus given time to Governments and populations to take precautions against it. The want of uniformity in the quarantine regulations of different countries has proved a great impediment to international commerce. The new sanitary regulations recently adopted by the Ottoman Government, and which have been founded upon the decisions of the International Sanitary Conference of 1866, have been very hurtful to Russian commerce. The Russian Ambassador at Con-

Constantinople has frequently directed attention to this fact. General Ignatieff, in 1872, intimated the necessity of revising the question of quarantine, and suggested a Conference for this purpose, either in London or Vienna. The Emperor approved of the suggestion, and the Minister for Foreign Affairs immediately took measures to realise the project. The representatives of the different foreign powers at Constantinople were consulted on the subject, and they recognised fully the necessity for a revision of existing quarantine regulations and for the establishment of some degree of uniformity among the different powers in their action with regard to them. The Porte was then addressed, and it assented to the suggested Conference, Vienna being chosen as the place for the meeting.

Pending the Conference, however, the Porte expressed a desire to convoke an International Commission at Constantinople to examine certain propositions of Dr. Bartoletti, Inspector-General of Quarantine in Turkey, relative to precautionary measures against the importation of plague and other contagions from Persia. The Russian Government assented to this Commission, but at the same time insisted that it should not set aside the proposed General Sanitary Conference at Vienna.

Meanwhile, the Russian Minister of Foreign Affairs took advantage of the International Medical Conference of last year to bring before it the questions of the measures to be adopted against invasions of cholera and the convocation of an International Conference on Quarantine to be held also in Vienna. At the same time the question of the Conference was submitted anew to the Austro-Hungarian Government, and met with a very favourable reception from it. This Government addressed invitations to the different Governments interested in the Conference to take part in it, and the greater number have assented. Each Government will be represented by two delegates, preferably medical men. The Conference, moreover, will be authorised to invite to its assistance any persons possessing special knowledge, official, scientific, epidemiological, or other, which may assist their deliberations. A programme of proceedings is being prepared.



## NOTE ON CHOLERA HOSPITALS.

A CORRESPONDENT has written to us relative to the observations on cholera hospitals contained in the article on "The Influence of Aggregation on the Sick in Hospitals for Infectious Diseases," in the *Practitioner* for February. He contrasts them with Dr. Sutherland's strongly-expressed objections to hospitals for the treatment of cholera in his report to the General Board of Health on the cholera epidemic of 1848-49, objections consistent with our correspondent's experience. We think that our correspondent has misapprehended the present position of the question. If the adoption of cholera hospitals rested solely upon the results of treatment in them, Dr. Sutherland's opinion would probably largely obtain. But, as Dr. Sutherland himself admitted in the report to which our correspondent refers, "there are circumstances under which some sort of hospital accommodation will perhaps always be required during cholera epidemics" (p. 129). Accordingly, in the Instructions to Guardians issued by the General Board of Health in the cholera epidemic of 1854, drawn up, we believe, in so far as the medical duties were concerned, by Dr. Sutherland (certainly with his approval; see his Report on Epidemic Cholera in the Metropolis that year, p. 86), we read: "It will be right to provide some hospital accommodation, not only for those who may be taken ill in the streets or at a distance from home, but also for those who live in unhealthy places or crowded rooms. The hospitals should be well ventilated and well drained, and should be near the epidemic locality." Here the circumstances which call for hospital accommodation are stated. To the like effect were the Instructions to Guardians issued by the Privy Council in the epidemic of 1866, and drawn up by Mr. John Simon. These instructions say that "when the Medical Officer of Health recommends, the Vestry or Board shall, with as much despatch as practicable, provide fit and proper accommodation for the reception of such patients as have no home, or cannot properly be treated at home, and may with advantage to themselves be removed," &c. Hospitals, in fact, have been found to be one of the necessary provisions for dealing properly with a cholera outbreak, and, as such, the requirements of such hospitals have to be discussed. The report of the Epidemiological Society on cholera hospitals in 1865, shows that the leading men in the profession were quite agreed on this point. Sir Thomas Watson, Sir William Jenner, and Sir William Gull each expressed a decided opinion that cholera hospitals should be provided in the event of an epidemic. In respect, however, to the treatment of cholera in hospitals, the earlier experiences must be corrected by the later experiences to which the author of the paper which has called forth our correspondent's letter refers. The teachings of the epidemic of 1848-49 as to the conditions under which cases of cholera ought not to be removed, have not been lost upon the profession.



# THE PRACTITIONER.

MAY, 1874.

Original Communications.

## THE "SPECTATOR" AND THE VIVISECTION QUESTION.

BY THE EDITOR.

NOTHING in the whole course of the miserable discussion respecting vivisection, which has been going on for some time past, has struck us with such painful surprise as the line of policy pursued by our contemporary the *Spectator*. That journal has in the past distinguished itself by many high qualities as to which it would be a mere impertinence for us to speak, but it had especially recommended itself to physicians and other men of science by the candid manner in which physiological and other scientific problems were canvassed in its pages. Above all, a certain striving after truth and impartial justice was displayed by the *Spectator* even with regard to questions where the personal predilections—theological or metaphysical—of the writers were evidently wounded by the facts or the arguments brought forward by physiologists or by physicists.

Unfortunately, in this vivisection controversy, our contemporary has altogether abandoned these estimable principles of conduct. Accusations of cruelty of the gravest kind have been made, without any sufficient inquiry into the facts, against men whose feelings of humanity, as those who really know them can

testify, are exceptionally tender; the onslaught against Professor Schiff, in particular, is one of the most scandalous pieces of injustice that we have ever witnessed. It is true that the *Spectator* seems in the first instance to have been misled by the raw head and bloody bones romancing of Dr. de Noe Walker, a gentleman whom Professor Schiff has publicly declared that *he never knew*, and who cannot, therefore, have been a student of the Florence laboratory in that sense which would alone give him any accurate information respecting the practices of the Professor. We cannot, however, admit this as an excuse, because the editor of the *Spectator* is well acquainted with many medical men of high standing, who could have told him the exact facts about the Florence laboratory, and about Professor Schiff. What these facts are have been abundantly shown by the letter of Dr. Herzen to the *Times*, by the collapse of the prosecution at Florence,<sup>1</sup> and by the communication of Dr. Hake to the March number of the *Practitioner*. The truth is that Professor Schiff, instead of being a wanton inflicter of needless suffering upon animals, has been most scrupulously careful to avoid such infliction, and that this characteristic is in accordance with his well-known and marked benevolence in all other relations of life. Dr. de Noe Walker only escapes the reputation of an intentional libeller by being universally considered a very rash person. That story of his, about the skins of the animals being so hacked that the skin-dealer would not buy them, will not be forgotten in a hurry.

The latest exploit of the *Spectator*, however, far exceeds in extravagance anything which had gone before, and is, in plain language, a Philistine declaration of war to the knife against science. Dr. Wickham Legg, a well-known physician and pathologist, is candidate for the demonstratorship of morbid anatomy at St. Bartholomew's. The post is very important to him, and (without any disparagement to possible competitors) he is known to be eminently deserving of it. But it happens that in the course of his scientific researches he has performed the experiment of making a biliary fistula in as many as sixteen cats.

<sup>1</sup> It was proved, as we had no doubt from the first, that the cries of the animals, of which Professor Schiff's neighbours complained, proceeded from animals who were merely tied up, not operated upon at all.

It is, of course, perfectly well known to our readers that this operation is a necessary preliminary to researches which are being carried on, as they probably will be carried on for some time to come, in order to settle the many vitally important questions connected with the functions of the bile. The operation itself is done under chloroform, and (once the biliary fistula is established) then ensues a period of decline from inanition which there is not the smallest reason for believing to be more painful than, say, the last few days of an aged person who sinks out of life from simple failure of the vital powers. Whether this be an accurate estimate or not, of the exact amount of suffering endured, however, matters little to our present argument; since the operation is one which some of the most illustrious and humane of physiologists have recommended as a necessary means to discovering facts which are of great importance, and which it may take a long time to completely collect and classify. Therefore, whether the operation be right or wrong in the abstract, Dr. Legg has only done what he had every reason to think was right, and most assuredly he did not inflict any great or terrible amount of suffering.

Such being the nature of Dr. Legg's conduct, it is scarcely credible that the *Spectator* should make it the ground of a public appeal to the Governors of Bartholomew's, not to elect him to a post which is necessary to his progress and success in the profession, and which is quite unconnected with the subject of vivisection. This action of our contemporary is nothing less than downright persecution, as cruel and unjust as could well be. It will scarcely succeed, we should hope, even with the ignorant public. As for medical men, they will know very well how to resist a tyranny which is all the more offensive because it is carried on in the name of humanity, and which recalls to mind the obscurantist fury of those who in earlier times persecuted Vesalius and Harvey and many others to whose labours we owe the whole solid foundation of modern physiology, and a multitude of benefits which have accrued to mankind in the way of alleviations of human suffering.

We would ask the editor of the *Spectator* what can he suppose, after calm reflection, would be the result of such a crusade as he is desirous of inducing the professional philanthropists to

undertake against vivisection? Very certainly it would be this:—the careless Gallios among physiologists would promptly enough surrender; they would readily give up a kind of research which, so far from being the child's-play (but for its cruelty) that our contemporary evidently thinks it, is one of the most laborious and difficult, and (in the matter of any applause or pay to be gained) the most thankless, which it is possible to undertake. As for the *enthusiasts*, the people whom the *Spectator* really dreads, they would simply do what scientific experimenters have had to do on many previous occasions, they would pursue their researches in secret, and it would be perfectly impossible to prevent their doing so. Moreover, if an occasional detection took place the experimenter would cheerfully face his accusers and the public, supported by the firm conviction that his labours were necessary to that progress of knowledge which can alone obtain substantial mitigation of the sufferings, not of men only, *but of the lower animals themselves*. The *Spectator* seems to forget that veterinary medicine and surgery are still in an infantine state: and that if beasts are ever to derive even as much succour from the medical art as men now receive, it can only be through a similar development of knowledge to that which has led to improvements in human medicine. So far, experiments on animals have been made chiefly for the sake of indirect inferences to be gained respecting the physiology or the diseases of men; but in the future, experiments on living animals must unquestionably be performed for the sake of establishing a sound system of animal pathology and treatment.

In short, and more especially in view of the questions which are involved in that last word, *treatment*, it is best at once to tell our friends who choose to arrogate to themselves the monopoly of humane feelings, that they fight against the inevitable. In spite of the dubious medical authorities who have been quoted in depreciation of the value of vivisection, it is a mere truism among real students of science that progress in physiology, and especially in the means for alleviating disease, must largely depend on experiments made on living creatures. It would ill become a journal which professes to assist, however humbly, the great enterprise of placing therapeutics on a

rational basis, to pass this question in silence, or to speak in any uncertain tone respecting it.

While, however, we assert uncompromisingly, not merely the right but the duty of medical men to perform experiments on living animals, we are not among those who think it advisable to avoid the discussion of particular questions respecting the limits within which the practice of vivisection should be restrained. It has somewhat surprised us that the other medical journals have not already spoken out on this matter, for in private conversation we have found that there is a very general agreement of feeling among those of the profession who are best qualified to judge of these matters. And the first thing which the profession at large would undoubtedly say is, that doctors are not such fools or hypocrites as to pretend that vivisection has never been abused by scientific men of a certain class. They would point with reprobation, and indeed with horror, to the experiments of Mantegazza, in which the effects of extreme pain upon the organism were deliberately tested. They would state with perfect truth that such experiments are wholly alien to the principles and practice of the professors in European laboratories generally: and that they represent a cruelty of disposition which is due, not to the natural attitude of the scientific mind, but to a national indifference to brute-suffering which is unfortunately too manifest, even at the present day, in southern countries, though great improvements have taken place of late years.

In the second place, the medical profession would, we are certain, emphatically assert that experiments, whenever that is possible, should be rendered painless from first to last, by the use of anæsthetics.

But thirdly, they would no doubt assert that the occasional and *incidental* infliction of pain, when the object of discovery is highly important, neither can nor ought to be forbidden. In order to ensure that this power is not abused, the humanitarians would do well to assist, instead of hindering, the most public and complete acknowledgment of the legitimacy and importance of vivisection *per se*. When they will do this, when proper laboratories are provided, and an apparatus of experimentation which shall suffice for the cultivation of the



exceedingly important field of inquiry which lies open before us, then we do not doubt that the practice of vivisection will be rendered less, and not more, objectionable than it is at present. It is a gross error to suppose that the eminent and highly cultured men who would be attracted to our chairs of physiology would be likely to waste animal life or produce needless animal suffering; for nothing is so hateful to the true scientific instincts.

Lastly, we do not doubt that a very brief discussion between the leading physiologists would settle upon a satisfactory basis the extent to which vivisections ought now to be repeated for the mere purpose of teaching. We are equally convinced that this must occasionally be done, and that the occasions would be very rare if once a code of principles were laid down by common agreement.

We cannot conclude without a remark which the humanitarian fraternity would do well to ponder. Since the days of Tartuffe there probably has never been witnessed such a glaring piece of insincerity as the outcry against vivisection for scientific purposes, by people who would not for a moment hesitate to inflict the agonies of the chase (far more dreadful than were ever inflicted by the scientific vivisector) upon an unfortunate fox or hare, for the mere purpose of amusement. It is impossible to go beyond that, in the direction of what the *Spectator*, with a most unlucky disregard of this fact, has called Pecksniffianism, an epithet which it invented to describe the moral attitude of the apologists of vivisection.

## A METHOD OF TREATING TINEA TONSURANS.

BY EDGAR A. BROWNE,

*Surgeon to the Eye and Ear Infirmary, and the Dispensary for Skin Diseases,  
Liverpool.*

THE publication, by Dr. Samuel Gee, of a method of treating tinea tonsurans (resembling that commonly in vogue, with the exception that a solution of sulphocyanide of potassium is substituted for the sulphurous acid), induces me to draw attention to a plan which has proved very satisfactory in my hands of late years.

The general principles on which the treatment of this disease must be founded are pretty generally admitted to be, destruction of the parasite, alleviation of irritation, and modification of the soil. Of these, the last, if fully carried out, would alone constitute an ideal mode, as rendering the skin fungus-proof would include the rest; but in actual practice we are frequently obliged to content ourselves with an imperfect fulfilment of this condition, and to rest our hopes of success on means calculated more directly to injure the vitality of the parasite. In the attainment of this object the second condition is too often violated, and the results of our treatment by the ordinary parasitocides are commonly worse to behold and nearly as tedious to cure as the original ringworm.

Parasitocides are generally applied in the form of lotions, dabbed vigorously into the patches, and kept in contact by soaked pads of lint covered with oiled silk, &c. The objections to the plan are, the frequency with which the application must be renewed, annoying both to patient and attendant, the difficulty (or, say, impossibility) of making nurses understand

the importance of maintaining a constant and not an intermittent atmosphere over the part, and the trouble of keeping the pads in position, on dependent portions of the scalp such as the occipital region.

In a certain number of cases where one or other of these drawbacks becomes prominent, the following plan will be found useful. A margin of healthy hair is to be cut quite short or shaved round the patch. A brisk rubbing with the *ol. picis rect.* or some similar hydrocarbon is the next stage, and the reddened and saturated patch is to be thickly dusted with a powder composed of tannin, iodine, and gum arabic. This is to be moistened with a few drops of the oil, and gently but firmly pressed into the skin with the end of a small cork. Repeat the process till the whole patch is covered with a layer of paste about an eighth of an inch thick, and then allow it to dry. The firm hard scab thus formed may be left undisturbed for three or four days, when it should be moistened, scraped off, and reapplied. No home dressing is required in the intervals.

This mode of treatment is not easily applied to patches of more than about an inch in diameter, as the artificial scab is apt to crack and fall off in pieces, nor is it at first received with much favour by parents of the upper classes on account of its somewhat suggestive appearance. Simple as it is, parents and nurses can seldom be trusted to make the application,—the surgeon should act as dresser.

The advantages of the method are the long interval between the applications contrasted with what can be had with any evaporable dressing in common use; the absence of irritation; the prevention of auto-inoculation by combing, &c., owing to the solid roof formed over the fungus; and the avoidance of bandages, pads, impervious night-caps, or other easily disarranged contrivances.

It is at least as rapidly effectual as any other plan with which I am acquainted.

## ON "REST" IN THE TREATMENT OF CHEST AFFECTIONS.

BY FREDERICK T. ROBERTS, M.D., B.SC., M.R.C.P.

*Assistant Physician to University College Hospital, and to the Brompton Consumption Hospital.*

*(Continued from p. 188.)*

BEFORE proceeding to consider the advantages to be derived from rest in certain other pulmonary affections, I may just mention a case which is at present under my care at the Brompton Hospital, and which seems to me to indicate very clearly the beneficial results to be derived from strapping the affected side in cases of pleurisy, after the manner already described, even when there is a considerable amount of fluid effusion. The patient is a woman, aged 34, with a marked hereditary phthisical history, and she has suffered from the local and general symptoms of pulmonary phthisis for some years, but lately had been much better. She is supposed to have caught cold, and a return of the chest symptoms resulted from this, with a severe stitch in the side. When she came to the hospital, she presented an aspect of serious illness, with much emaciation. Breathing also was much distressed. On examination of the chest, the remains of old disease were found at both apices, but in addition there were the signs of considerable pleuritic effusion on the right side—so much, indeed, that I feared fixing the side could be of very little service. However, I resolved to try the effect of this treatment, and it has proved most satisfactory. The pain and distress in breathing ceased immediately the application was made, and the patient, on each occasion that she visits the

hospital, alludes with gratitude to the marked relief she experienced.

But what is more important is, that absorption commenced speedily, so that in a week the fluid had greatly diminished in quantity, and at the end of a fortnight it was entirely removed. I have a strong conviction that strapping the side did in this instance check the effusion, and hasten and aid its absorption very materially. There was nothing in the internal medicines employed which could have at all promoted this object. I may add that the patient's general condition has improved considerably.

There are two other affections in which mechanical applications to the chest prove very serviceable, and these demand a brief notice.

PLEURODYNIA.—This complaint, which is often a very painful and annoying one, may generally be relieved completely by applying two or three strips of plaster firmly round the side over the seat of pain. The emp. roborans is a good form of plaster for these cases. It is desirable that the practitioner should himself see that the application is properly made, and that it is not left to the patient, as the result entirely depends on its being accomplished in an effectual manner.

PNEUMOTHORAX.—My own experience of this morbid condition, in medical practice, is limited to those cases in which a cavity in the lung has given way in the course of phthisis, and consequently air has escaped into the cavity of one of the pleuræ. Two classes of cases occur, differing materially as to their severity and importance. (*a*) In one set, owing to the existence of extensive adhesions or agglutinations in connection with the pleura, only a limited quantity of air escapes, and the accumulation is confined within a small space, so that respiration is not materially interfered with, and the chief symptom is the pain in the side, which has come on suddenly and is generally very considerable. Physical examination reveals the signs of local excess of air. In these cases I have found satisfactory strapping of the side give immediate relief; while as a rule no further ill-effects were experienced from the mishap, the air, I suppose, becoming absorbed, and the opening from the lung



closed. (b) In the other class, which are far more grave, adhesions either do not exist at all, or they are insufficient to prevent the air from pervading the entire pleural sac, which therefore becomes more and more distended, the chest-wall being driven out, and the lung compressed. Under these circumstances breathing becomes greatly embarrassed; and what is often very distressing to the patient is, that there is a feeling of great distension and want of support over the side, with much difficulty in coughing. In cases such as this the application of some mechanical apparatus over the affected side is eminently serviceable, especially if employed at an early period after the perforation has taken place. This apparatus needs to be very firm and resisting, and therefore the best form is that of plaster, covered over with two or three layers of strips of bandage steeped in a mixture of gum and chalk, as described when speaking of pleurisy. Its effects are, that it limits the escape of air, if applied soon, by restraining the respiratory movements; or, if much air has already accumulated, it gives considerable relief by affording a sense of support over the side, rendering breathing more easy, and enabling the patient to cough more effectually. Should the pleural sac be much distended, however, the best plan of treatment would be to remove the air from the pleura by means of the Aspirateur, and then immediately put on the apparatus I have mentioned.

With regard to the two ordinary acute affections of the lungs,—viz., ACUTE PNEUMONIA and ACUTE BRONCHITIS—there are certain points which call for notice in connection with the subject of rest. In both these diseases it is very important that the air inhaled should be as pure as possible, warm and moist, so that it may not cause any local irritation of the affected tissues. These conditions are obtained by keeping the patient in a room maintained at a tolerably high temperature, well ventilated however, and employing some means for allowing steam to pass into the apartment and mix with the atmosphere.

In the treatment of *acute pneumonia* there are certain matters which deserve more consideration than they usually receive. In the first place the patient ought to be kept in *as complete a state of general rest* as can be obtained; and I particularly

call attention to the necessity of being very careful as to the "physical examination" of these cases. This should always be done with as little disturbance to the patient as possible, due regard of course being had to its being satisfactorily carried out, and it should not be performed more frequently than is absolutely required. I believe no small degree of injury is inflicted in some of these cases by the patient being examined many times in the day, or by several individuals, and there is a particular danger of this in hospital practice. Another point of importance is to *check all disturbing actions in connection with the respiratory organs*. The breathing is of course always hurried in pneumonia, but it is often much more so than it need be; and I have found not uncommonly that patients can *voluntarily limit the number of respirations*, if instructed to do so, and that this has at least added considerably to their own comfort. Further, they should *not be troubled to speak*, if this can possibly be avoided. And, lastly, it is often of much importance to employ measures for *checking cough*. This symptom is in many cases very distressing in the early stages of acute pneumonia; it causes much suffering, while it does not fulfil any useful purpose at this time, and must necessarily cause much local disturbance. Hence, if the cough is at all severe in the early period of this disease, I think that remedies which tend to relieve and subdue it are decidedly serviceable, though they require care in their administration.

The employment of some *external application for restraining movements* over the part of the chest corresponding to any portion of lung which is the seat of pneumonia, suggests itself. So far as I have had opportunities of judging of the effects of such applications, they do not appear to be, as a rule, particularly beneficial, and they interfere with the use of other external remedies which are probably more valuable in this disease. Still, in the ordinary cases of acute idiopathic pneumonia affecting the lower lobe of one lung, I think the plan of strapping the corresponding region of the thorax deserves further trial. I believe, also, that I have found it useful in certain cases of inflammation affecting one apex.

*Acute bronchitis* demands, in my opinion, very different management from that of pneumonia, with respect to the points

just considered. Here the object should be to *disturb the patient and the respiratory organs*, within due limits, rather than to encourage a state of rest. I wish to lay much stress upon, and to call particular attention to this statement, as my conviction is that much harm not uncommonly results from an opposite course being pursued. One of the chief objects in the treatment of bronchitis should be to try to *empty the bronchial tubes* of the morbid materials which form in them, and thus to prevent their accumulation.

The dangers resulting from the air-passages becoming filled up with secretion are well known and are sufficiently obvious, especially in the case of children and aged or very weak patients. It is worthy of note, however, that the accumulation may go on very insidiously, without any visible signs of danger, until it is found that the smaller tubes are extensively filled with a more or less viscid secretion, which cannot be expelled. I believe that some of the cases of so-called "capillary bronchitis" are merely due to neglect of the precaution to encourage the emptying of the bronchi in an ordinary attack of bronchitis.

These remarks will explain the *rationale* of what follows. In cases of bronchitis of any extent, it is desirable to *rouse the patients*, rather than allow them to remain quiet; while *frequent examination* of the chest is to be commended, partly because it generally excites cough, and thus promotes expectoration, and partly because it will enable the practitioner to detect any tendency to undue accumulation of secretion. In conducting it, however, it is necessary to be careful not to expose the chest too much or for too long a time. Considerable advantage may be derived also from directing the patient to *take deep and full inspirations* from time to time, by which the expulsion of the sputa is decidedly aided. But what is more important than all, is the way in which *cough* should be managed. It is but too common a practice to give indiscriminately in all cases of bronchitis medicines of a sedative and narcotic nature, with the view of alleviating this symptom, forgetting altogether that it is probably serving a useful and necessary purpose. This practice may, and I believe does, cause no small amount of harm; and I would insist very strongly on the desirability of *encouraging cough* rather than subduing it, as *the rule of practice* in acute

bronchitis. Of course there are cases in which it is more severe and frequent than is required for the discharge of the expectoration, and then it is proper to try to alleviate it, examination being made from time to time, in order to ascertain that the lungs are satisfactorily emptied. But in most instances the opposite course is indicated, particularly if the bronchitis is at all extensive; and the more the smaller tubes are involved, the more imperative does it become to use every means for promoting the expulsion of the morbid secretions. There are a few simple yet practical suggestions bearing upon this matter, which I would venture to submit:—1. A patient suffering from bronchitis should not be allowed to lie horizontally, but the head and shoulders should be well raised and firmly supported—in bad cases, indeed, a half-sitting posture being best. 2. On no account should prolonged sleep be permitted; and this needs attention *by night* as well as by day. I have known cases in which, owing to the neglect of this rule, dangerous accumulation of secretion has occurred, and this requires the more notice, because it is often difficult to make the friends of patients understand the necessity for disturbing them when comfortably asleep, which they consider a cruel procedure. My plan always is, in any serious case, to give definite instructions as to the intervals at which the patient should be awakened—these of course being longer or shorter according to the requirements of the case, judged by the quantity of secretion formed. One of the great evils of administering narcotics for the relief of cough in this disease is, that it encourages sleepiness when the contrary state is decidedly to be desired. In not a few instances, I believe, the fatal termination so frequent in bronchitis is partly the result of this treatment, especially in the case of children, for whom the tinct. camph. co., syrup papav., &c., are very favourite remedies. 3. The patient should be instructed to cough at more or less frequent intervals, and to do it effectually, *i.e.* so as to expel a quantity of the sputa, and thus keep the air-tubes as free as possible. This is the object for which the practice of disturbing the patient from sleep is recommended, who should be directed to give a good, vigorous cough on each occasion. In order to render the cough more powerful and effectual, the patient should be made to sit up, and be told to

breathe deeply. Further, although no mechanical appliance outside the chest can have any influence on bronchitis, I have found that the act of coughing is often materially aided by applying a bandage tolerably firmly round the abdomen, and extending high enough to include the thorax for a short distance above its lower margin. This also renders the act less painful and fatiguing; for it otherwise generally causes considerable muscular pains and soreness about the sides and abdomen.

Respecting those cases in which *pneumonia* and *bronchitis* are more or less combined, the course to be pursued must be determined by the exact conditions in each case. In none of them, however, can the principle of rest be applied to the extent that it is in simple pneumonia; they all need a certain degree of disturbance, after the manner described under bronchitis, and the more the tubes are filled, as judged by physical examination, the more frequent this disturbance ought to be.

(*To be concluded.*)



## ON TISSUE-DESTRUCTION IN THE FEBRILE STATE, AND ITS RELATIONS TO TREATMENT.

BY DR. ANSTIE.

### PART II.

I CONTINUE my records of cases in which the general course of the urea-elimination has not been coincident with, but has been opposed to, the course of the temperature.

CASE III. is that of a patient in the University College Hospital (Mary Howard, aged 16), affected with rheumatic fever. The observations of temperature and of nitrogen excreted were carried out for me by Mr. Hicks, the combustion process being always employed. Unfortunately during the first two days the urine could not be collected: this is to be regretted, as the temperature was very high; in the latter hours of the second day of observation it was  $105^{\circ}6$  and  $105^{\circ}4$ . However, the urine was carefully collected on the next day: the mean temperature, determined from five observations (at 2.45 A.M., 6 A.M., 11 A.M., 5 P.M., and 9.30 P.M.), was  $103^{\circ}98$ ; the nitrogen discharge, reckoned as if it were all urea,<sup>1</sup> gives the low figure 122.3 grains of the latter. The volume of the urine was only eight fluid ounces (224 c.c.) On the following day the mean temperature on five observations was  $103^{\circ}3$ ; the volume of water was eighteen ounces (504 c.c.); and the nitrogen, reckoned all as urea, gave the amount of the latter as 295.5 grains. On the fifth day of observation the temperatures had become quite normal, the urea-discharge was 465.5 grains; and on the two

<sup>1</sup> Of course this involves a slight error, as a small part of the nitrogen represented lithates, creatine, &c.

following days, when the temperatures were more than a degree below the normal level, the urea is reported at 307·5 and 339·6 grains, respectively. Of course, as already said, these "urea" figures are got by doubling the amounts of the total *nitrogen* from urea, lithates, pigment, creatine, and creatinine), which last is the essential matter for our inquiry. The deficiencies of this case are as obvious as they were unavoidable, but the facts are nevertheless valuable as far as they go. The patient, judging from her age, and from the urea-ratio after defervescence, might be supposed to excrete as much as 300 to 350 grains a day, on a mixed diet in health.

The next case which I have to record is that of a girl named A. W., aged 17, who was a patient under Dr. Green, in Charing Cross Hospital.<sup>1</sup> The record commences on the eighth day of typhoid fever; the mean temperature on this day was 102°·33, and the urea elimination amounted to 433·3 grains. The case was a thoroughly mild one as regards its general features: still the temperature was distinctly and characteristically febrile during the whole second week of fever. On the ninth day the mean temperature was 103°·6; on the tenth, 103°·4; on the eleventh, 102°·3; on the twelfth, 101°·1; on the thirteenth, 100°·1; on the fourteenth, 100°·1. By the middle of the third week the temperature had become normal. Here the urea-discharge is accurately known during ten days corresponding to the period above named; it makes an average of 343·7 grains per diem. In calculating the significance of this we have to inquire as to the amount of nitrogenous food-supply, and as to this we can give a fairly precise account. 120 oz. of milk daily were allowed, and seem to have been completely consumed. The Charing Cross Hospital milk had been analysed for me, about this time, by Mr. Frederick Hicks, by means of Parkes's modification of the soda-lime combustion process: the average of two samples (taken on different days) gave 1·945 grammes nitrogen to the

<sup>1</sup> I take the first opportunity which has offered itself of acknowledging the great kindness of the physicians of Charing Cross Hospital in permitting me to examine the urines of their patients. And I especially desire to recognise the kindness and ability with which Mr. Taylor, house physician to the hospital, has assisted me in assuring the accuracy of the collection and measurement of the urines, and also in giving me the daily fluctuations of temperature. It is impossible for me to thank him too warmly.

pint, according to which 120 oz. of the milk contained about 180 grains of nitrogen. Now, the average daily urea-discharge in the above case has been stated at 343·7 grains, which would contain rather less nitrogen than was ingested in the milk. Thus far, then, there is nothing to point to excessive combustion of tissues; and as regards the relation between the urea and the temperature charts, it is interesting to observe that they by no means correspond: thus, in the twenty-four hours reckoned as "tenth day" the mean temperature was as much as  $103^{\circ}\cdot4$ , while the urea stood at the low figure of 269·69 grains, or more than 100 grains less than was passed on the thirteenth day, when the mean temperature was  $100^{\circ}\cdot1$ ; diet remaining exactly the same. No stimulants were used during the pyrexial period of this case.

The next cases to be recorded are two which I was very lucky to be able to observe at the same time. By what is certainly a rare chance, two cases of acute primary lobar pneumonia, in previously healthy adult males, came into Bouverie Ward, Westminster Hospital, on the same day, under the care of my colleague, Dr. Fincham. They both ran a perfectly simple course, and no alcohol was taken by either. One patient was named M—, the other W—.

In M—, the phenomena of intense primary pneumonia were developed with picturesque distinctness: the florid countenance, rapid breathing, and peculiar sputa were well developed on the third day (first of observation); the mean temperature of this day was  $105^{\circ}$ : the urea-discharge was only 272·57 grains. The temperature continued exceedingly high until deferescence, which occurred, with great simplicity, on the sixth day of pneumonia. The average of six days gives a daily elimination of 450 grains of urea, a very moderate range for pneumonia, especially when contrasted with the following temperature record, in which the readings will be seen to be very high. First day of observation, mean temperature  $105^{\circ}$ ; second day,  $103^{\circ}\cdot7$ ; third day,  $104^{\circ}\cdot05$ ; fourth day,  $101^{\circ}\cdot6$ ; fifth day,  $100^{\circ}$ ; sixth day,  $99^{\circ}\cdot6$ ; seventh day,  $98^{\circ}\cdot7$ .<sup>1</sup>

<sup>1</sup> Not only did the pyrexial period come well and quickly to an end in this case, but the local changes disappeared without the slightest litleh. It was curious, however, that the dyspnoea remained for some days later, to a degree which was puzzling and at first disquieting. All passed off well in the end.

In W——'s case the mean temperature was considerably less high, while the urea-discharge, on the other hand, averaged 515 grains daily during seven days. The quantity of nitrogen which this represents is 262·5 grains, as against a daily nitrogen discharge, in M——'s case, of only 225 grains; while the average mean daily temperature of the former was more than one degree Fahr. lower than that of the latter. The ingoing nitrogen was the same in both cases during the pyrexial period, about 90 grains daily. Both of these cases must be regarded as marked examples of intense simple pneumonia with a comparatively small amount of urea-discharge for that disease. If we consult the statistics of urea-discharge in "pneumonia" which are to be found in books, we shall find that in acute cases a very much higher rate of elimination has commonly been observed, even when the diet has been almost purely a starvation ration. For instance, in Parkes's work there are twelve separate observations of pneumonia, of which three are due to the author, and the rest are by Brattler, Moos, Wachsmuth, Metzger, Zimmermann, Alfred Vogel, and Julius Vogel; in only three of these twelve does the daily urea amount to less than 600 grains, in the lowest it is 516, and in four it is above one thousand grains. From Senator's work I quote six other cases, in which the daily urea-discharge was respectively 521,<sup>1</sup> 362,<sup>2</sup> 536,<sup>3</sup> 580,<sup>4</sup> 620,<sup>5</sup> and 585<sup>6</sup> grains; a series which gives a lower view of the average urea-discharge of pneumonia than the series cited by Parkes would indicate. On the other hand, my own experience is very positive. Taking all my cases of pneumonia together, I have examined fifteen (in which no alcohol had yet been given) for two, three, or more days consecutively during the pyrexial period; sometimes, but not always, including the critical days. The result is an average of about 700 grains daily, for the whole group, which, however, contained a great variety of cases. The youngest patient was 17, the majority

<sup>1</sup> Second to seventh day; Brattler.

<sup>2</sup> Second to eleventh day; *absolute starvation*. Wachsmuth.

<sup>3</sup> Four days, *not including crisis*. Huppert.

<sup>4</sup> Second to eighth day: only about 45 grains of nitrogen were taken, in food, during the seven days; two and a half times this quantity was expelled in feces and sputa. Huppert and Riesell.

<sup>5</sup> Third to ninth day; Unruh.

<sup>6</sup> Third to ninth day; Senator.



were between 20 and 40. I have already published one case<sup>1</sup> in which a young man (in St. Thomas's Hospital) passed on the average of three days (ending with the sixth of pneumonia) an average of 1,089 grains, the sixth day giving 1,348 grains; here I had the advantage of Mr. Hicks's valuable assistance, who examined for total nitrogen by the combustion process.

From all these considerations it will be evident that the two cases of pneumonia in the Westminster Hospital, which are recorded above, were decidedly mild examples as regards the matter of nitrogenous-tissue-destruction. At the same time, as regards high fever and dyspnoea, they both presented (M—— especially) the conditions which are popularly connected with severe pneumonia. My colleague, Dr. Fincham, however, did not think that either of them needed active treatment of any sort, and the results abundantly justified his opinion.

Such facts as the above evidently possess an important significance, and one that cannot be too early or too thoroughly investigated. I have related six examples of acute disease (in the present and in the March number), in all of which a regular (and in four out of the six a very severe) pyrexial process was accompanied by an unusually moderate elimination of urea. Especially remarkable are the cases of Hughes and Jefferson (*Practitioner* for March), and of the pneumonic patient M——, whose case was related just now, on account of the extraordinary divergence between the temperature and the rates of urea-discharge which they exhibit. I may add that the six cases which have now been detailed, and also five others with analogous conditions, have all been collected by me within the short space of four months during which I have been investigating acute diseases in several London hospitals. That so large a number of cases of the kind should have been collected by one observer in so short a time in the scanty intervals of heavy work of other kinds, is, I think, strong ground for presumption that there is a considerable proportion of cases of acute pyrexial disease which are not attended by any large excess of nitrogen-discharge over nitrogen-ingoing. In every one of the eleven cases of this kind which I have observed, the disease, however severe some of its external characters

<sup>1</sup> In a clinical lecture, *Lancet*, March 21, 1874.



might be, proved to be mild in its effects upon the constitution, and capable of being successfully treated without stimulation on the one hand, or any special anti-febrile treatment of an older type on the other. It will be found, when I proceed in future papers to discuss the action of alcohol in pyrexia as tested by my special observations, that there is a remarkable contrast between the type of pyrexial disease which is, and that which is not, accompanied by a high discharge of urea.

At present I do not wish to do more than direct attention to the simple fact of the coincidence, in the cases observed by myself, between the mildness of diseases, which from the amount of fever might have been supposed dangerous, and the low rate of formation of urea: and to state that this condition of low urea-formation is in perfect opposition to anything that I have ever seen in fatal or nearly fatal cases. It is evident that we have here an exemplification of the enormous differences which may exist between the maladies of patients who are affected with (nominally) the same disease, and even with a similar intensity of the disease were we merely to judge by certain striking outward signs, such as high temperature and rapid pulse and breathing. Surely we have in this fact, a warning of the most significant kind against that sort of treatment which is supposed to rest on what is called "experience" and to ignore the more exact methods of research. The application of this reflection to the case of alcoholic treatment in pyrexia is, as I hope to demonstrate in future papers, of the utmost consequence. In fact I am not without hope that the dangerous vagueness which has everywhere and in all ages characterised the statements of medical writers respecting alcoholic stimulation in febrile diseases, is about to be replaced by a practical exactness of knowledge on the subject, which will be found not inferior to that which we at present possess respecting any remedy whatever. The combined use of the urea-research and of observation of the pulse with the sphygmograph will be shown to have yielded results of which it is difficult to exaggerate the practical value in prognosis and in treatment. But this must be reserved for the forthcoming papers on the medicinal uses of alcohol in fever, to which my papers published in the last few months have only been intended as preliminary.

## ON THE ACTION OF PURGATIVE MEDICINES.

BY T. LAUDER BRUNTON, M.D., SC.D.

*Casualty Physician and Lecturer on Materia Medica at St. Bartholomew's Hospital.*

BEFORE entering upon their use, it will be well to consider the question—How do certain substances act as purgatives?

It is generally believed that most purgatives increase the number of the stools and render them more fluid in a double manner; firstly, by stimulating the intestine to increased peristaltic action, and secondly, by inducing a discharge of fluid from its mucous surface, and thus to some extent washing out its contents.<sup>1</sup> Some purgatives, such as aloes, are supposed to act almost entirely by stimulating the peristaltic movements, their effect on the secretion from the intestine being almost *nil*; while others, like bitartrate of potash, are supposed to induce a very free secretion from the mucous membrane, while they have so little influence upon the peristaltic movements that the fluid poured out from the intestinal wall after their use may remain in the torpid intestine so long as to be again reabsorbed.<sup>2</sup> Others again, like croton oil, are supposed to increase the flow of liquid into the intestine, and at the same time to stimulate the peristaltic movements.

This view of the action of purgatives is the one generally held in this country. It is supported by several French authorities, but is rejected by some of the most eminent German pharmacologists.

All are agreed in believing that the action of many purgatives is due to their power of quickening peristaltic action, but

<sup>1</sup> Pereira's "Materia Medica," vol. i. p. 247; Stillé's "Therapeutics and Materia Medica," vol. ii. p. 404; Ringer's "Therapeutics," 3rd ed. p. 154.

<sup>2</sup> Garrod's "Materia Medica," 3rd ed. 1868, p. 401.

several German authors are inclined to regard increased peristalsis as the only, or almost the only cause of purgation, and to deny that there is any increased flow of fluid from the intestinal walls. They consider that purgative medicines, by quickening the peristaltic action, cause the contents of the intestine to be hurried along and expelled *per anum* before there has been time for the absorption of their fluid constituents. Instead, therefore, of the stools being firm and consistent as in the normal condition, they are loose and watery like the *faecal* matters which one usually finds in the small intestine on post-mortem examination. By making a fistulous opening in the ascending colon of a dog, Radziejewski, the distinguished pharmacologist, whose untimely death is much to be deplored, has found<sup>1</sup> that the intestinal contents, when poured from the small into the large intestine, almost exactly resemble the stools produced by the use of purgative medicines. He therefore, and several German authors who follow him, attribute the watery condition of the stools observed after the administration of vegetable purgatives, such as castor or croton oil, to increased peristalsis only. The objection may be raised that the stools produced by *elaterium*, for example, are more watery than the contents of the small intestine usually are; but this can be readily met. For it is not merely the peristaltic action of the large intestine which is quickened so that the *faeces* are expelled in much the same condition as they entered it. The movements of the small intestine are also accelerated, so that little absorption can take place in it, and its contents will therefore leave it in a more watery condition than usual, and being quickly hurried through the large intestine, will produce a liquid motion.

This explanation may seem satisfactory enough in regard to the action of vegetable purgatives, and of such mineral ones as calomel; but it hardly explains the effects of salines, such as bitartrate of potash or sulphate of magnesia. These are allowed by Buchheim<sup>2</sup> to have an additional action besides that of increasing peristalsis. They retain water with great avidity, they diffuse slowly, and by thus preventing the water which is taken with them or swallowed shortly afterwards from being

<sup>1</sup> Reichert u. Du Bois-Reymond's *Archiv*, 1870, p. 95.

<sup>2</sup> Buchheim's *Arzneimittellehre*, p. 136.

absorbed, at the same time that they quicken the intestinal movements, they wash out the whole alimentary canal from end to end, in much the same way as a simple injection washes out the rectum. A large quantity of fluid is normally poured into the intestine by the liver, pancreas, and intestinal glands, and this alone, according to Kühne,<sup>1</sup> is greater than the amount expelled in the most profuse diarrhœa. When, in addition to this, the quantity of fluid ingested by the mouth is taken into consideration, it seems perfectly unnecessary to believe that any increased flow of fluid takes place from the intestinal walls. Moreover, direct experiments seemed to show that purgatives did not increase the flow of fluid from the intestinal walls. Such a flow might be of two kinds: it might consist of a transudation from the blood-vessels, as supposed by Schmidt,<sup>2</sup> or of a secretion from the intestinal glands. In the former case it would contain a considerable quantity of albumen like the fluid in ascites or pericarditis; while in the latter, albumen might only be present to a very slight extent. A consideration of the structure of the intestine alone is sufficient to show the improbability of a direct transudation of fluid from the vessels; but Radziejewski<sup>3</sup> has set the matter at rest by examining the composition of fæces before and after the use of purgatives, and proving that the stools produced by them do not contain albumen to anything like the amount they ought to do if transudation fluids were present in them to any considerable extent. The most decisive experiments, however, were those which were first made by Thiry by means of the intestinal fistula which bears his name. These seemed to show in the most conclusive manner that purgatives neither increased the flow of fluid from the intestinal walls by transudation nor by secretion. In order to discover exactly what went on in the intestine, Thiry conceived the idea of isolating a portion of it and attaching one end of this piece to an opening in the abdominal walls while its nerves and vessels remained uninjured, and the whole piece was as nearly as possible in a normal condition. He therefore divided the jejunum or ileum in two places, a few inches apart from each other, sewed up one end of

<sup>1</sup> Kühne, "*Lehrbuch der physiologischen Chemie*," p. 151.

<sup>2</sup> C. Schmidt, "*Characteristik der epidemischen Cholera*," Leipzig, 1850, p. 90.

<sup>3</sup> Radziejewski, *op. cit.* p. 75.



the piece thus isolated, and attached the other end to the wound in the abdomen. The short *cul-de-sac* thus formed remained attached to the mesentery and received its vascular and nervous supply as usual. The divided ends of the intestine were then sewn together, and the continuity of the alimentary canal restored.<sup>1</sup> This is represented in the accompanying figure (Fig. 1), where B indicates the place where the piece C D, which originally lay between A B and B E, has been cut out, and the two ends of A B and B E sewn together so that the alimentary canal is again complete, though a few inches shorter than before. F, G, D, is the abdominal wall, and *a* and *b* are the vessels and nerves in the mesentery.



FIG. 1.

The little bag of intestine C D can be easily reached from the outside of the body, and the result of any experiment upon it readily ascertained. It apparently remains in a perfectly healthy condition, and when tickled with a feather readily secretes intestinal juice. But a purgative medicine introduced into it neither increases the secretion nor causes transudation from the vessels, although the drug produces brisk purgation if administered to the animal by the mouth. Thiry<sup>2</sup> in his experiments used croton oil, senna, and sulphate of magnesia. Schiff<sup>3</sup> repeated them with aloes, jalap, and sulphate of soda; and Radziejewski<sup>4</sup> with croton oil and sulphate of magnesia. All these observers obtained a like negative result. Further proof seems superfluous to show that purgatives act only by accelerating peristaltic action, and not by increasing the flow of fluid from the intestinal wall; and I have not only believed but have taught this, till the publication of some experiments of Moreau,<sup>5</sup> their verification by Vulpian, and the results I have myself obtained on repeating them, have led me to alter my opinions.

These experiments were made by opening the abdomen of an animal, and tying four ligatures tightly round the small intestine a few inches apart from each other, so as to isolate three por-

<sup>1</sup> Thiry, "Sitzungsbericht der Wiener Academie," 1864, vol. L. p. 77.

<sup>2</sup> Thiry, op. cit. p. 95.

<sup>3</sup> Schiff, "Nuove ricerche sul potere degerente," &c. H. Morgagni, July 1867, p. 5.

<sup>4</sup> Radziejewski, op. cit. p. 35.

<sup>5</sup> Moreau, "Archiv G n rales de M decine," August 1870, p. 234.



tions of intestine. A purgative medicine was then injected by means of a subcutaneous syringe into the middle part, and the intestine being then returned into the abdominal cavity, the wound in the abdominal parietes was sewn up. A few hours afterwards the animal was killed, and on examination the middle portion of intestine, into which the purgative had been injected, was found full of fluid, while the portion on each side was completely empty. All three pieces having been equally empty at the commencement of the experiment, and all three having been placed under exactly the same conditions, we cannot attribute the copious secretion into the middle loop to any other cause than the action of the purgative injected into it. Moreau's experiments have been repeated by Vulpian,<sup>1</sup> and I subjoin the notes of the results which I have obtained.

EXP. I.—A cat was chloroformed, and an incision about  $1\frac{1}{2}$  inches long made through the abdominal walls in the middle line about the umbilicus. A coil of small intestine was drawn out, and four ligatures tied tightly round it so as to isolate three loops of intestine. One-hundredth of a drop of croton oil mixed with one drop of alcohol was then injected into the second loop by means of an extremely fine Wood's syringe. (The quantity of croton oil was obtained by thoroughly mixing 1 part of oil and 99 of alcohol.) The intestine was then replaced, the wound sewn up, and the animal allowed to recover from the chloroform. About four hours and a quarter afterwards it was instantly killed by a single blow on the head with a hammer, and the intestine examined:—

Loop 1.	Length $3\frac{1}{16}$ inches	. . .	Contained 7 minims of fluid.
„ 2.	„ $1\frac{7}{16}$ „	. . .	„ 20 „ „
„ 3.	„ $3\frac{1}{16}$ „	. . .	„ 10 „ „

EXP. II.—A cat was operated on in the same way as the first, and  $\frac{1}{16}$  of a drop of croton oil with 10 drops of alcohol was injected into a loop of intestine which, as in the former case, lay between two others likewise isolated by ligatures. About four hours and a quarter after the operation the cat was killed in the same way as the first:—

Loop 1.	Length $2\frac{1}{16}$ inches	. . .	Completely empty.
„ 2.	„ $3\frac{1}{16}$ „	. . .	Contained 80 minims of fluid.
„ 3.	„ $3\frac{1}{16}$ „	. . .	Empty.

<sup>1</sup> Vulpian, "Bulletin Général de Thérapeutique," tome lxxiv. 1873, p. 522.

EXP. III.—Made in the same way as the preceding ones. One drop of croton oil and 9 of alcohol injected into loop No. 2. Four hours and a quarter afterwards :—

Loop 1.	Length	4 $\frac{1}{2}$ inches	:	:	:	Empty.
„ 2.	„	5 $\frac{1}{2}$ „	:	:	:	Contained 110 minims of fluid.
„ 3.	„	6 $\frac{1}{2}$ „	:	:	:	Empty.

Just about the middle of loop No. 2 the mucous membrane for about 1 $\frac{1}{2}$  inch was thickened, much reddened, and inflamed.

EXP. IV.—Made in the same way as the preceding ones. Ten drops of croton oil were injected into loop of intestine No. 2. I am not quite certain that the whole of the 10 drops found their way into the intestine, as the oil passed very slowly through the fine hollow injecting needle, although considerable force was used. The syringe at one instant became detached from the needle, and a little oil escaped. I tried to guess the right amount, however, and injected it afterwards. Four hours and a quarter after injection :—

Loop 1.	Length	6 $\frac{1}{2}$ inches	:	:	:	Contained 155 minims of fluid.
„ 2.	„	5 $\frac{1}{2}$ „	:	:	:	„ 180 „ „
„ 3.	„	5 $\frac{1}{2}$ „	:	:	:	„ 75 „ „

The fluid, as measured, was not quite accurate, for a tapeworm was present in the intestine, and parts of it helped to swell the apparent bulk of the fluid.

In the middle of loop No. 2 the mucous membrane was much inflamed for about two inches or rather more. This is the part with which the oil would come in contact after its injection through the intestinal wall.

The mucous membrane of all three loops, as well as that for four or five inches above the upper and below the lower loop, was much thickened, and the lumen of the intestine partially filled with a glairy fluid. Above and below these parts the intestine was firmly contracted and natural, just as when the injection was made. The mucous membrane in all the coils was somewhat pale, as also at the thickened parts outside. At the other parts where it appeared unaltered, its inner surface was of a yellow colour, probably from adherent biliary or fecal colouring matter.

EXP. V.—A cat was experimented on as before. A small

quantity of elaterin (probably about  $\frac{1}{10}$  of a grain) suspended in 30 drops of water was injected into loop No. 2. About four hours and a quarter afterwards:—

Loop 1.	Length	$5\frac{1}{8}$ inches	. . .	Contained 60 minims of fluid.
„ 2.	„	$6\frac{1}{8}$ „	. . .	„ 110 „ „
„ 3.	„	$5\frac{1}{8}$ „	. . .	Empty.

In all three loops, as well as for five or six inches beyond the loops, the mucous membrane (or whole intestinal wall?) was pale and somewhat thickened.

EXP. VI.—A cat was chloroformed, an incision made in the abdominal walls, and three loops of intestine isolated by ligatures. Into the middle one (No. 2) about two grains of gamboge made into an emulsion, with about 60m. of water, were injected at 11.15. The wound was then sown up, and the animal allowed to recover. About four hours afterwards the cat was killed by a blow on the head and the intestine examined:—

Loop 1.	Length	$4\frac{1}{8}$ inches	. . .	Empty. Mucous membrane yellowish on the surface.
„ 2.	„	$5\frac{5}{8}$ „	. . .	Contained 185 minims of yellowish turbid fluid with numerous flocculi. The surface of the mucous membrane was slightly paler than in No. 1.
„ 3.	„	$6\frac{3}{8}$ „	. . .	Empty. Colour like No. 1.

EXP. VII.—The experiment was performed on a cat in the same way as the previous one. Into the middle loop of intestine about one grain of jalapin in a small quantity of spirit (proof) and water (equal parts) was injected. The intestine was examined about four hours afterwards. The cat seemed sleepy, and the respiration appeared to be impeded by fluid in the respiratory passages:—

Loop 1.	Length	$5\frac{1}{8}$ inches	. . .	Quite empty. Surface of mucous membrane normal.
„ 2.	„	$6\frac{1}{2}$ „	. . .	Contained 17 minims of tenacious fluid. Surface of mucous membrane moister than in No. 1.
„ 3.	„	$5\frac{1}{2}$ „	. . .	Mucous membrane moist. Covered with bloody mucus.

EXP. VIII.—The experiment was made in the same way as the preceding ones. Into the middle loop, No. 2, about 7 grains of sulphate of magnesia dissolved in 105 minims of water were injected. Into each of the side loops 105 minims of water were injected. The intestine was examined about four hours afterwards:—

Loop 1.	Length 5 inches	. . .	Quite empty.
„ 2.	„ 7½	„ . . .	Contained 320 minims of fluid. This was of a pale amber colour and glairy consistence, mixed with flakes of whitish mucus. Not the slightest trace of congestion was noticeable. Mucous membrane was quite natural in No. 2.
„ 3.	„ 5½	„ . . .	Quite empty.

EXP. IX.—The experiment was conducted like the others. Into the middle loop of the cat's intestine 85 minims of a saturated solution of sulphate of magnesia were injected. On examination four hours afterwards, the middle loop, which was 7½ inches long, contained 425 minims of fluid. The other two loops were quite empty.

EXP. X.—The experiment was conducted as before. Into the middle loop of the cat's intestine about 90 minims of a saturated solution of sulphate of magnesia was injected. The loop was about 6 inches long. After about five hours the loop was found to contain about 250 minims of fluid. The loop above it contained a little bloody mucus, the one below it was entirely empty.

These experiments show that croton oil, elaterin, gamboge, and sulphate of magnesia all cause a copious secretion from the intestine. Jalapin did not do so in the single instance in which it was tried; but I am not quite certain that the whole of it went into the intestine, as it formed a resinous mass which I had considerable difficulty in getting to pass through the nozzle of the syringe. The fluid contained in the intestine after the use of the other purgatives appears to be a secretion, not a transudation, for it does not contain much albumen as a transudate would do. In Exp. VIII. it amounted to about 42 minims, and in Exp. IX. to about 56 minims per square inch of intestine

acted on by the purgative. The greatest secretion was caused by sulphate of magnesia; next came croton oil, elaterin, and gamboge; while jalapin stood last of all.

Such positive results as these seem to prove that purgatives do cause a flow from the intestinal wall, quite as conclusively as experiments with Thiry's fistula do the opposite; and as the conditions under which the purgatives act on the intestines more nearly approach the normal in Moreau's than in Thiry's experiment, there can be but little doubt that purgatives produce a decided secretion of fluid from the intestine, as well as accelerate peristaltic movements.

*(To be continued.)*



## ON OBSTETRICAL EFFORT DURING CHLORO- FORMIC ANÆSTHESIA.

BY DR. CHARLES JAMES CAMPBELL,

*Late House Surgeon to the Maternité, Paris, and late "Chef de Clinique  
Obstétricale" in the Paris Faculty.*

THE cause of the apparent freedom of parturient females from the dangers which accompany anæsthesia, even when it is carried, as may unexpectedly happen, beyond the limits of safety, is a question which has occurred to the mind of every physician. The number of obstetrical cases subjected to anæsthesia (either at their physiological termination or for surgical interference) and anæsthetised, under the most varying circumstances, by medical men of experience or not, rash or prudent, ignorant of rules or armed with the best precepts, favoured or not by individual idiosyncrasies,—the number of such cases, I say, is so very considerable, and the statement of the fact so universal, without there having occurred until this day (at least to my knowledge) a single accident which could be strictly attributed to the use of anæsthesia, that the question naturally occurs as to what can be the cause of this perfect harmlessness of the method, or of such tolerance of anæsthesia, in parturient females.

As to the method itself, I believe that, independently of the smallness of the doses and of the law of intermittence which directs, in obstetrics, the administration of such doses—the double condition which constitutes the very essence of obstetrical anæsthesia—the accoucheur enjoys, through the nature of things, a much more favourable position than the surgeon, in respect to the

possible danger of anæsthesia. At the same time as the intermittent character of the pains, which we seek to abate, becomes more marked; at the same time as their periodical return invites us to model thereupon our anæsthetic intervention, we notice, at a given moment of the labour, the supervention of a new and special phenomenon, which does not appear upon the physiological scene before the second part of every parturition, or period of expulsion, which is, as I have elsewhere established, the true period of election for the administration of chloroform. This special phenomenon is none else than the EFFORT which the woman makes for expelling the infant out of the utero-vaginal canal. Let us now see what part this effort can play in the explanation of the anæsthetic tolerance peculiar to parturient women.

The physiological act of effort is always present for the termination of normal labour. It may be, indeed, profoundly modified by certain general conditions of health which produce amyosthenia (eclampsia, paralysis, grave fevers, phthisis, &c.), or by local conditions consisting of a want of proportion or adaptation between the fœtus and the passages which it has to go through (abnormal presentations, vicious conformations, &c.); but, except these cases, every labour implies an effort, which, during a certain time or the whole time, is exerted in its integrity. We must not forget, besides, that the above exceptional conditions constitute, as I have often had cause to mention, formal counter-indications to the employment of anæsthesia, and that consequently they have no reference to the special subject of anæsthesia in normal labour.

It is when obstetrical effort is about to supervene—that is, when the fœtal ovoid is preparing to engage in the uterine orifice, already completely or almost completely dilated, and to pass through it—that anæsthesia finds its formal indication. So that it may be put down as a rule, that when, all other conditions being good, there exists a clear and powerful effort of expulsion, we may, without waiting further, allow or concede anæsthesia, which will then be carried on according to the rules laid down long since by our obstetrical authorities.

It is, in fact, the exertion of effort, an intermittent act like uterine contraction, synchronous with this latter, and, above all,

coexistent with all possible expulsion, since it is the motor of it,—it is this effort which creates in favour of the parturient female the remarkable harmlessness of anæsthesia, and in favour of the accoucheur a degree of security drawn from the knowledge of the fact that there exists a kind of compensation exerted by the function itself on distant organs, and which, I think, had hitherto escaped the notice of observers in the study of obstetrical anæsthesia.

Let us therefore examine what are, independently of the local phenomena of labour, the distant effects of effort. Physiology, properly so called, will first furnish us with a knowledge of its general character, whilst obstetrics will teach us what are its special characteristics.

“At the moment of effort,” says Professor Bécclard,<sup>1</sup> “pulmonary circulation is remarkably impeded. The air enclosed in the lung being compressed, furnishes at this moment an obstacle to the arrival of blood in the capillary network. The blood accumulates in the right heart, then in the veins; and, should the effort be prolonged, the veins of the head, face, neck, and upper extremities are distended. Hæmorrhagic accidents may then supervene in the situation of the brain.”

“Effort is, as regards circulation, accompanied by the physiological phenomena which follow upon suppression of respiration. As this suppression can last only a certain time, so effort is only intermittent, and is diminished from time to time, in order to operate an expiration or inspiration. From these modifications of respiration there result changes in the number of the beatings of the heart, and consequently in the depletion of the larger blood-vessels.”

As to the value of effort<sup>2</sup> during parturition which has reached the period of expulsion, every accoucheur knows full well what it is. Besides, physiology again gives us the following information in relation thereto:—

“The pains of the woman become extremely violent. To the contractions of the uterus are afterwards joined those of the abdominal muscles, and also those of the muscles of the trunk. The powerful contraction of the muscles induces the effects of

<sup>1</sup> *Traité élémentaire de Physiologie*, par J. Bécclard, 4th ed. 1872, pp. 670, 1129.

<sup>2</sup> *Dictionnaire de Littré et Robin*, art. “Effort,” p. 493.

violent effort. Jerking respirations follow each other in rapid succession, in order to consolidate the thoracic cage and furnish fixed points to the contraction of the muscles; the face is injected; the heart beats violently."<sup>1</sup>

We all know that it is to the combined efforts produced by the simultaneous contraction of the muscles of the abdomen and diaphragm, assisting the uterine muscle, the action of which, at first isolated and independent, is not joined until towards the second period of labour to its auxiliaries, that parturition is due. The auxiliaries of the uterine muscle, though they derive their innervation from a different source, obey, nevertheless, as being respiratory muscles, that double influence which anæsthesia with difficulty affects, and the result of which ends in the general excitement of the patient, the rise of her pulse, the acceleration of her respiratory movements independently of the moment of effort, the increase of heat in all the organism, and finally the definitive expulsion of the fœtus from the maternal passages.

And here I wish to draw special attention to the results produced by effort on distant organs in the parturient female. We observe that there then exists on all the periphery of her body, but especially in the situation of the head, a state of hyperæmia, due, it would seem, to a double cause originating in the degree which the effort has attained. Thus, during the first period of effort, and from the moment the thoracic cage is fixed, the face is seen to become red and congested. The fact is, that the woman has received at the outset, towards the cephalic extremity, *intus et extra*, only the blood thrown out by the heart and the arteries, the activity of which was increased by the general circulatory tension due to the effort. But as soon as the effort becomes prolonged, we immediately observe that to the primary red congestion is added a cyanosed aspect due to the venous stagnation produced by the obstacle which exists to the return of venous blood to the right heart. There results from these two periods, ordinarily very near each other when the efforts are great, a state of arterio-venous congestion of the inter-cranial circulation, as marked as that which is observed to discolour the extra-cranial periphery, the face and neck.

<sup>1</sup> Bécларd, "Physiologie," 4th ed. p: 1129.



I will not attempt to investigate in these pages the part belonging to each of these sorts of congestion, presenting a different sanguineous type; nor the question of making out whether the arterial impulse dominates the venous stagnation, whether there is a compensation between any quantity of chloroformic arterial blood and any quantity of venous blood which, returning from the nervous centres already reached by the anæsthetic agent, must contain a large proportion of it.

M. Paul Bert has seen arterial blood more furnished with oxygen during chloroformisation than in the normal condition. Professor Claude Bernard thinks that this may be simply an exceptional state. In all this there is a whole physiological chemistry mysteriously going on between the nervous cells, their surrounding medium, and chloroform, and which our illustrious masters in physiology can alone investigate and elucidate. All that I believe I know is, that during the exaggerated cardiac action which accompanies effort, the woman being placed in the usual horizontal position, much more favourable than the vertical or sitting, there is a greater flow of arterial blood to the head, whilst the quantity of venous blood remains about the same on account of the venous stagnation. From which it follows, that in virtue of the same law which enables us during chloroformic accident, by practising sudden and more or less protracted inversion of the individual (upside down) to hyperæmiate mechanically the brain deprived of blood, so, in cases of regular labour where chloroform has been given in small intermittent doses, we see effort (that effort which should never be wanting) take care to furnish of itself, at regular and never protracted intervals, a sanguineous current to those nervous centres which, in labour, can never, through the above fact, become constantly or continuously anæmiated. The nervous cell affected by the chloroform, to the degree which produces anæsthesia, is constantly maintained through this intermittent stimulant nutrition, in a state to be able to resist even temporary death, which has not time to take place in the course of such a series of efforts periodically congesting the brain. Every risk for the parturient woman is therefore periodically, at short and constantly renewed intervals, necessarily warded off—which is the same as to say that in reality it is definitively removed, and



allows the act of parturition to terminate with almost no pain to the woman, whilst it offers the greatest security to the accoucheur.

Thus, on each one of the efforts which characterise the second period (or that of expulsion) of physiological labour, there occurs hyperæmiation of the nervous centres, the cerebro-spinal as well as the sympathetic. This momentary excitement of the nervous cells modifies, by diminishing, the stupefying action of chloroform, which we know manifests itself to the eyes of the physiological experimenters by a state of anæmia and collapse of the blood-vessels. As soon as the passing congestion ceases, relative anæmia is again produced, and if the dose of chloroform is still sufficient, anæsthesia goes on again until the arrival of the next current of blood; and so on till the end of the accouchement, if it is thought necessary to continue the inhalations. Had it been possible I should have annexed to this paper the relation of a case in which the linking of these alternating phenomena of hyperæmia and anæmia was, I think, very conclusive.

Consequently there is thus established between the anæsthetic anæmia and the hyperæmiation of effort a veritable intermittent antagonism against the action of chloroform, which is, so to say, fractionised, in such a way that the influence of one hyperæmiation has scarcely had time to disappear before the arrival of the influence of the next hyperæmiation. We shall see, however, that there may be an interval of complete anæsthesia (without any new addition of chloroform). But this interval is short, and as chloroform may be withdrawn during a required time, there is no risk of a fatal collapse, as at least one hyperæmiation has taken place since the last approach of the anæsthetic agent. I shall have occasion presently to allude to a means of procuring an effort artificially without the help of the uterus, and of thus affording a supplement of one by cerebral hyperæmiation, which may be created at will if there arises any indication to this effect. What is most admirable in these natural tactics of contractions and concomitant efforts during the second period of natural labour, is that we may rely to a minute on their regular recurrence. Their flux and reflux might almost be compared to those of the tidal waves which wash the beach at short, regular

intervals, coming and going, and always leaving the sands moist, even under the rays of the hottest sun.

Here a question of much practical interest incidentally presents itself. We are scarcely able to widen the interval between two efforts, when labour is progressing regularly. The intensity of an almost surgical anæsthesia may sometimes occasion a delay in the return of effective contraction, but this delay is easily removed, either by withdrawing chloroform or through the action of this practical fact—there exists in such cases a sort of functional compensation; the efforts are less frequent under the influence of chloroform, but they are intensified and are more efficacious; and the woman, without being conscious of it, pushes on the child, which rapidly crosses the perineum which has been rendered more supple. It is thus we may explain the greater rapidity of some labours subjected to chloroform, whilst before anæsthesia they seemed to languish on account of the pains which enervated the woman, and the more so as the perineal resistance is too difficult to surmount when the efforts are kept in through fear of suffering or weakened by the simultaneous emission of uncontrollable cries.

Is it possible to realise an effect contrary to the one just described, and which takes place independently of our control? Can we, notwithstanding the existing anæsthetic condition, create an effort, cause more frequent efforts? Yes; but without exerting any influence on the duration of labour. We can do so, when needed, with the object of producing more frequent hyperæmiation of the brain, a series of congestions that will be salutary, and we do so by soliciting the efforts of the respiratory muscles independently of uterine action. The means of awakening muscular action is (1) to order, with the voice, the anæsthetised female to push, which she will do if she can hear; (2) to establish reflex action by means of pressure exerted with one or two fingers on the posterior part of the perineal plane. These two means employed simultaneously seldom fail to occasion an effort which has but small influence on the progress of labour itself, since the action of the uterus is supposed to be absent at the moment when, between two true contractions, we desire to produce a supplementary effort in view of congesting the brain, as in cases of protracted anæsthesia.

If it were not irrelevant to my subject, I would enter here into various considerations of interest on obstetrical anæsthesia during the first period of labour. At this period, veritable effort, that which arises from contraction of the respiratory muscles, is not yet exerted. *Uterine contraction* is then the only hyperæmiating agent of the nervous centres, as is proved on the one hand by the *coloration of the face* in women who only begin to suffer and do not complain of much pain, and on the other hand by the *frequency of the pulse* during the time of uterine contraction.<sup>1</sup> Is there in this degree of cerebral hyperæmia, connected with the slight increase in the heart's action, during the first period when true effort is absent, a sufficient guarantee for the use of anæsthesia at the outset of labour? This may be, as far as I may judge from some cases which I have observed; but I must add that in these instances, occurring in excited and restless females, there are other general causes of cerebral hyperæmiation besides the heart's action. I therefore conclude that it is safer to reserve chloroform for the second period, when the effort is real and complete. Chloral seems to be the best sedative of pain in the first period, when suffering is moderate. At any rate, it was of interest to notice the presence and effect of *isolated uterine effort* at the outset of labour.

The only logical and practical inference to be drawn from the as yet limited number of exceptional cases of anæsthesia during the first period is, that the delay which anæsthesia at this period causes in the return of uterine contraction, and the lessened intensity of each individual contraction, show that the slight anæmiation of the brain produced by the heart's action under the influence of uterine contraction alone, is scarcely sufficient to furnish the cerebro-spinal, and especially the ganglionic, nervous centres with enough nervous power to prevent their excitability from being extinguished. Chloral, though its action is not yet sufficiently well defined, seems therefore, in small doses, better applicable than chloroform to this period. As a hypnotic, it would soothe general sensibility without hindering, it appears, uterine action. Indeed, according to certain observers, the action of the uterus would even be more marked and singular.

<sup>1</sup> Cayeaux, 8th ed. p. 273.

Dismissing these intercurrent remarks, I again express my firm conviction that these intermittent hyperæmiations, due to obstetrical effort which produces a flux and reflux of nourishing fluid, bathing the nervous centres, keep up the integrity of these latter, and permit them to resist the deleterious effect of protracted chloroformisation. It is in this, precisely, that resides the difference between obstetrical anæsthesia and surgical anæsthesia. In individuals who have been operated on, there exists no essential condition capable of producing at short intervals those currents of blood which can repair the anæmia of chloroform. We can therefore easily understand that, with a mode of administration differing so widely from ours, and having besides to deal with moral and physical idiosyncrasies particularly affected by the disease from which they suffer, surgeons meet with more frequent occasions of mischief from chloroform than do accoucheurs, whose method of administration is in itself a precaution against accident. This remark, however, never should prevent accoucheurs from being faithful to the plan of small doses, administered intermittently, and almost always during the second period only.

In obstetrics, this guarantee against the dangers of anæsthesia should always be taken, as it is linked with the very nature of things, provided always that labour in its second period be not interrupted by anæsthesia itself. Now, physiologists and accoucheurs have always been agreed in stating that if anæsthesia, with ether or chloroform, somewhat delays the recurrence of uterine contractions (a fact which is not always constant or invariable), it never influences them to the degree of annihilating them, which is most important, as the removal of uterine contraction would be a fatal objection to obstetrical anæsthesia. In like manner, it was stated that muscles otherwise innervated than the uterus were also unaffected by the resolving influence of anæsthesia, because they were the auxiliaries of respiratory work, and consequently of effort, which is but a modification of this latter. It is needless to observe that should these essential conditions of normal labour and regular anæsthesia be wanting, the employment of chloroform must be withheld or suspended. Manual or surgical interference, or simply expectancy, would be resorted to for terminating such an irregular accouchement.



From the various considerations which precede, I think I may safely draw the following conclusions:—

The periodical return of OBSTETRICAL EFFORT which characterises the second period of normal labour, its return at intervals almost invariably identical in the same case, and which may be estimated at from three to five minutes on an average, produces congestion or hyperæmiation of the nervous centre, which is intermittent like the return of effort.

In accouchements submitted to anæsthesia, this expulsive effort, with its immediate and remote effects, appears to me to exist in about the same degree as in cases where chloroform is not employed. Occasionally there has been a wider interval between two succeeding efforts. The contrary has likewise been noticed; and some observers have stated a certain increase in the intensity of effort.

If, on the other hand, it is shown that anæmia of the nervous centres which accompanies anæsthetic sleep predisposes to syncope, of which death (either of the sensitive nervous cell or of the individual) may be a consequence, and may, in a given case, become real and definite instead of temporary and apparent, then it seems that the effects produced by effort (such as I have described it) are of a kind which can successfully oppose the obvious effects of anæsthetic anæmiation.

The nature of this effort is to congest the brain and to render it momentarily as vascular as it is normally in children, respecting whom Paul Guersant used to say that if chloroform were ever given up as regards adults, it should be retained for the surgery of childhood. It is the natural and remarkable vascularity of the nervous centres in children which enables them to bear so admirably all the degrees of chloroformism.

The existence, during the expulsive period, of cerebral hyperæmia of an intermittent form, caused by repeated return of effort, in cases subjected or not to anæsthesia, appears to me to explain, if not to show, the strange and consoling immunity (hitherto not sufficiently explained) which parturient women seem to enjoy, amongst all nations, in all countries, under the care of all accoucheurs, within the experience of the last quarter of a century, to the dangers of anæsthesia. Does not such an interpretation, applying to the totality of thousands of success-



ful cases, lead us to admit the existence of an *anæsthetic tolerance* peculiar to women in labour?

And if such a *tolerance* does exist, as I am led to believe, is it not of a nature, whilst observing all the reservations which I have noted previously, the examination of special aptitudes and idiosyncrasies, and of all the conditions necessary for its regular development,—is it not of a kind, I say, to give more confidence in the harmlessness of the means and in the security of the employment of the method, both for our patients, in the first place, and also for ourselves?

## Reviews.

*Therapeutic Action of Drugs on the Healthy System; an Index to their Therapeutic Value, as deduced from Experiments on Man and Animals.* By ALEXANDER G. BURNES, M.B., C.M. Univ. Aberdeen, and F. J. MAVOR, M.R.C.V.S., President Central Veterinary Society. London: Baillière, Tindall, and Cox, 1874.

WE have not laughed so heartily for a long time as in reading this book: but laughter was by no means the first emotion which it excited in our mind. The title-page, though occasioning us some misgivings, gave room for hope that we should meet with some truly original work in the succeeding pages: for nothing could be better than that an experienced veterinary surgeon and a young (and therefore presumably well-educated) graduate of Aberdeen should work together in experimental inquiries as to the action of medicines. As we read on, we felt a growing annoyance at the generally feeble and ineffective way in which every branch of the subject was treated, but more especially at the fact that the statements of the authors were evidently based on speculations of a metaphysico-homœopathic type, and that the so-called experimental observations by which these speculations should have been tested were of the flimsiest kind. Up to this point we had been so much engaged in examining the matter of the book that we had not noticed its style, beyond observing in a general way that as regards English grammar and construction, and the number and gender of Latin words, the authors had views of their own. But when we turn to the preface—*solvuntur risu tabula*, for surely never, since the immortal letter of Mr. Weller senior, announcing the death of Mrs. W. to his son Sam, was there such an extravagantly absurd performance. It begins in the first person, and it is signed by Dr. Burness; but as happened in the case of the Wellerian epistle, "the old'un will keep putting his oar in" in a way which is nothing short of distracting. It is a regular game of "puss in the corner" between "I," "we," and "he." The

amusement which we derived from the preface induced us to look a little more closely at the text of the book, and we have made a very fine private collection of literary gems. We need not trouble our readers with a detailed list of these; it will be enough to say that "*Aconitus Napellus*" is only one of many similar eccentricities in Latin; that "therapeutic acid" is an example of a recklessness of press-correction which is habitual; and that at page 168 we meet with the following passage:—"at the same time the milk given to the child was directed to be boiled first." Here we are wholly at a loss to know whether the caution given was against boiling the child before the milk, or against attempting to boil the milk after the child had taken it.

We should be glad if this book could be treated altogether from a jocular point of view; but that is not the case. One cannot treat as a mere joke a work on therapeutics, published at the present day, which starts with such high pretensions as those which are put forward by Dr. Burness and Mr. Mavor. It unfortunately happens that while the attention of the profession has of late been strongly directed towards the action of remedies, there has been as yet very little diffusion of knowledge respecting the modern methods of exact investigation in therapeutics. Hence it is quite possible that the crude statements contained in books like that before us might exercise a most unfortunate influence upon the development of opinion in the profession at large.

The first matter upon which we must remark is the threefold division which the authors attempt to establish between the Toxic, the Physiological, and the Therapeutic action of medicines. They affirm the existence of this division in very confident terms in their first chapter; but they offer, in that place, nothing which claims to establish the proof of it, referring their readers to the sections on special drugs for the complete demonstration. When we turn to the special sections, however, we find no fulfilment of this promise. The mere idea of such a division is sufficiently astonishing. One can understand, and indeed one must admit, the existence of the physiological and that of the therapeutic action of medicinal substances as perfectly separate and distinct. When we speak of the physiological action of a drug we may reasonably group, under that term, all the effects which the agent, whatever it be, is capable of inducing in an animal organism into which it passes; and it is obvious that the ground thus covered is not conterminous with the field of curative action, which is limited by various conditions, on the one side as to the quantity of the agent employed, and on the other as to the pathological state of the animal tissues and fluids which it is intended to remedy. But there is no rational

ground for dividing the toxic from the physiological action of a drug: such a distinction is wholly imaginary. We are not surprised, therefore, to find that the authors of the work before us get themselves into a hopeless muddle in the vain attempt to work out practical illustrations of their great principle. We should be sorry to be at all unfair to the authors, so we will take a specimen of their work which is certainly representative, being concerned with a very important drug—digitalis. Here we find, under the head of “Physiological Effects,” first of all a few lines of very vague description of the influence of small and of full doses, respectively, on a healthy person; then a description of a case of digitalis poisoning from Taylor; next a hurried and very inadequate reference to Eulenberg and Ehrenhaus’s experiments on frogs with digitaline, and then the following jerky and disconnected account of an experiment by the authors:—“To a colt was given by subcutaneous injection, at 2.43 P.M., two grains of digitaline; pulse 48. At 2.48, restless pawing the ground. At 2.53, restless, passed fæces. At 3.25, pulse 51. At 3.45, standing very quiet, tongue and buccal membranes slightly dry; pulse 42. At 4, still standing very quiet. At 4.15, looked sleepy, erection of penis. At 5.15, passed fæces again. At 5.35, still quiet, erection of penis. No other effects were observed in this case.” It is not too harsh a judgment if we say that authors who can consider such an experiment to be of the slightest value, or could report it in such slovenly fashion, have not yet come to understand the true principles of experimentation at all; and although a considerable number of researches with various drugs are recorded in this book, it will be found that in almost every instance they are so badly reported as to be untrustworthy, and very often they are quite irrelevant to the matter in hand.

But the most conclusive test of the authors’ competence for their task is to be found in the list of “cases,” which they give at the end of the book, to illustrate the “specific action of drugs.” We have read a good many bad accounts of cases in our time, but we do not remember anything quite like Messrs. Mavor and Burness’s clinical reporting. They give any number of histories neither better nor worse than the following:—“In the following case the patient had been subject to irregular action of the bowels, which he had endeavoured to remedy, unsuccessfully, by the use of various nostrums. Under the use of the following treatment, the deranged function was restored, viz.,—restorative doses of tincture of nux vomica and sulphur.” Probably never since the famous explanation of the power of opium to cause sleep by the statement that it possessed a *virtus dormitiva*, has any more absurdly unmeaning therapeutical statement been made. Unfortunately it is not at

all exceptional. The book is full of headlong assertions without the slightest proof to back them, and is from first to last a disastrous performance. We regret to have to speak so strongly, but it is really necessary to place every obstacle in the way of the manufacture of books like the one before us, at a time when it is becoming daily more evident that progress in scientific therapeutics can only be effected by those who are prepared to expend the most patient and devoted labours in research, and to take the greatest pains to express their conclusions in language which shall at once be clearly intelligible, and also convey precisely the conclusions, neither more nor less, which are logically deducible from the facts which have been observed. And there is one special reason which forbade us even to pass the work of Messrs. Mavor and Burness in silence, as we might otherwise have been inclined to do, viz., the extraordinarily favourable terms in which it has been reviewed by at least one of the leading medical journals. Such criticism, if not counteracted, must convey to foreigners the idea that English medical men are unfit to form a judgment of the value of therapeutical researches, and would naturally lead to a thorough distrust of the reliability of any new statements that might be made in this country respecting the action of medicines.



## Clinic of the Month.

**Local Treatment of Carbuncle.**—Dr. Peter Eade records the following case in which a carbuncle was successfully treated by local means. A gentleman, aged 70, but still strong and hale, had suffered for some weeks from asthmatic bronchitis, from which he had in great measure recovered, when he was suddenly attacked with pain and hard nodular swelling of the lower lip, near to its right extremity. Almost immediately a double festering pimple formed over the centre of this swelling, and the painful induration rapidly extended past the corner of the mouth to the adjacent cheek, and to a less degree along the substance of the lip, causing also considerable cedematous swelling of the whole of this part. On the second day severe constitutional symptoms manifested themselves; the tumefaction and hardness continued to increase, the festers over the original spot opened and were moist with dirty pus, the whole of the lower lip and right corner of the mouth and the adjacent cheek became tense and swollen, and it was quite evident that a carbuncle was forming on the right side of the face. Into the centre of the two holes which had formed, Dr. Eade now pressed with a probe some threads of lint soaked in a strong solution of carbolic acid, in oil (one part to four), and he also laid a piece of lint wet with the same over their apertures, so as to supplement the small quantity which the shallow sinuses would contain. A little smarting was complained of, but the application was repeated after a few hours, and again the following day. Almost at the end of twenty-four hours it could be perceived that a check had taken place in the morbid process, but by the next day it was plainly evident that the inflammation and induration were really beginning to subside. The carbolised lint was still carefully and scrupulously thrust to the very bottom of the small holes, and from this time no further spread of the disease took place; but, on the contrary, there was a rapid subsidence of the cedema, and in two or three days more little remained but some diffused swelling of

the lower lip, some tender induration at and around the seat of the original pimple and the ragged discharging opening which had formed at the site of the primary festers. The disease was therefore stayed, and in a few days more the patient was convalescent. Dr. Eade, from observation of this and kindred cases, believes that when it has been applied early it has plainly gone far to abort the disease; and when it has been commenced later, wherever it could be brought into contact with the inflamed and hardened tissue, there at least no further spreading has taken place, whilst swelling and tenderness have diminished, and dirty suppurating slough has quickly given place to florid healthy granulations. He entertains no doubt that if it could be brought sufficiently early into contact with the spreading disease, it would be quite competent to prevent its extension beyond the degree to which it had already advanced. Unfortunately, the acid appears to have little or no influence when applied over the unbroken skin, but directly it can be brought into contact with the diseased mass either by being inserted into the sieve-like holes, or by being applied to it after being laid open by incision, its beneficial action becomes at once manifested. (*Lancet*, March 28, 1874.)

**Traumatic Tetanus successfully treated by Calabar Bean.**—Dr. Cunningham of Campbelltown records the following case. A boy, aged seven, met with an accident, lacerating his left foot, on July 31. The tissues covering the great toe and first and second metatarsal bones were bruised. The wound was cleaned and dressed with a weak solution of carbolic acid. The injury proceeded without an adverse symptom towards recovery until August 11, when his friends stated that he had caught a cold in being carried from one room to the other. The patient complained that his throat was sore. There was nothing abnormal to be found about the throat; it was ordered to be kept warm and the patient directed to keep his bedroom. At 8 P.M. the boy could not open his mouth wide, the muscles on the back of the neck were tense, he was fretful and refused food. A mixture of bromide of potassium and chloral was ordered, but he took none. Next day he was much worse, and would take neither food nor medicine. Aug. 13.—A twenty-fourth of a grain of Calabar bean was ordered to be given every hour and a half, unless faintness should supervene; also sherry, milk, and chicken-broth. In the evening he was much worse: the jaws were completely closed; the whole body was rigid. Chronic spasms occurred every fifteen or twenty minutes, each being preceded by severe pain at the sternum. After each attack there were profuse perspiration and rapid pulse. A twentieth of a grain of the extract in solution was ordered to be given every hour. The next day, Aug. 14, he was slightly

worse, and the dose was increased to one-sixteenth of a grain every hour. Aug. 15.—A little sleep was obtained, during which his mother said he looked as if nothing ailed him. Aug. 16.—The masseters were slightly relaxed; about a fourteenth of a grain of the extract was given every hour. No improvement followed. On the 17th one-twelfth of a grain was given every hour. On the 18th there was slight improvement, but the tendons of the ham-strings were like bands of iron; he had had a short refreshing sleep. Aug. 19.—Improvement not marked. About one-ninth of a grain was given every hour; and as he had had no motion from the bowels for two days, a simple turpentine enema was given. From this date steady and rapid improvement set in, all the symptoms subsided, the dose of the extract was gradually reduced, and in a fortnight or so he was convalescent. (*British Medical Journal*, April 4, 1874.)

**Phlegmonous Erysipelas treated by the Aspirator.**—Mr. Fred. Hall records the case of a woman, aged 44, the mother of three children, who had for eight or nine years experienced a dull aching kind of pain over the sacrum and left gluteal region, which continued for variable periods. She underwent various treatment, both professional and by quacks, and latterly had applied some plasters which irritated the surface and caused pustules to appear. At the same time her health was depressed in consequence of one of her children falling ill. The pain in her hip increased. She was unable to turn in bed without much suffering. On examination the left inguinal glands were found to be tender and swollen; the left hip decidedly larger and rounder than the right one, and somewhat tender on compression, especially over the course of the sciatic nerve and about the great trochanter. No pitting from digital pressure was noticed, nor was any fluctuation detected. The movement of the hip-joint could be made without remarkable difficulty, except when rotation outwards was fully performed, when sharp pain was felt. Walking was accompanied by feelings of stiffness, and restraint and weariness were soon felt. The menstrual period had just been completed.

At night all the worst symptoms were intensified, the tongue was moist and clean, appetite tolerable, thirst slight, pulse 84–90, soft, regular, and steady; pulmonary and cardiac condition satisfactory. The case was regarded as a masked form of sciatica due to rheumatism. A lotion of oxide of zinc, glycerine, and emulsion of almonds was directed to be applied during the day, with a warm linseed poultice by night. Good diet, with quinine and sulphate of iron, were ordered. On December 10, a week after the patient had been seen by Mr. Hall, her condition was essentially unchanged: hydrate of chloral was given at night to

procure sleep. On the 20th she took carriage exercise, the jolting of which augmented the pain and caused distinct rigors. She became somewhat hysterical, urine containing an excessive amount of earthy and triple phosphates. On the 26th cedema of the left foot and ankle was observed, with eczema of the dorsum, which annoyed her much at night, and the patient became worn out with nocturnal pains, insomnia, and perspiration. On the 30th a consultation with Dr. B. W. Richardson was held, and a grooved needle was introduced, which showed the presence of pus. Dieulafoy's aspirator was determined to be used. Accordingly on the following day, distinct fluctuation being felt a little below and to the outside of the great trochanter, ether spray was applied, and a No. 3 hollow needle was passed through the opening made by the grooved needle and forced gently for about two inches downwards, the receiver of the aspirator being previously exhausted. On turning the tap, pus began to flow, at first sluggishly and afterwards more freely, and about six ounces were withdrawn. On removal of the needle the opening was dressed with carbolised resin ointment, and a fresh, warm, large poultice was placed over the hip. The operation, which only lasted five minutes and was perfectly clean, was described by the patient as being disagreeable rather than painful, and after it the hip was flatter, paler, and smaller. Two days afterwards a free escape of pus took place on removal of the poultices, and recovery took place steadily from this time forth, except that she had an attack of eczema over the whole service of the lower limb, which was appropriately treated and soon got well. On the 13th January she was convalescent and went into the country. (*Lancet*, April 4, 1874.)

**Supra-orbital Neuralgia treated with Subcutaneous Injection of Strychnia.**—A correspondent of the *Lancet* records the case of a gentleman who had for some time been resident in a climate in which ague was prevalent, and who had an attack of that complaint, followed by a severe attack of supra-orbital neuralgia. The latter was distinctly intermittent, and considerably affected the general health. When seen, he had eaten very little for eight days. A quinine treatment was adopted without a favourable result. Two minims of solution of strychnia, four grains to the ounce, were injected into the right arm. Two days afterwards he was found to have slept well; his appetite was better, but there had been some slight return of the pain, and at the time of the second consultation he felt the supra-orbital pain, though somewhat diminished. Two minims of the solution were injected, and the pain instantly disappeared. A third injection was made, and from that time the patient has been in good health, the injection in the last instance being followed up



by the administration of iron and quinine as double citrate. (*Lancet*, April 11, 1874.)

**Aspiration as a means of treating cases of urgent Retention of Urine.**—In a paper read before the Medico-Chirurgical Society of Edinburgh, Dr. Joseph Bell remarked that, on the first appearance of the aspirator in surgical practice, he felt that he had a means of easily and safely treating certain cases of retention of urine. The literature of the various uses of this instrument is already very extensive, and it has been used to diagnose and empty abscesses, cysts, cases of spina bifida, encephalocele, hepatic abscess, hydatid cyst, hernial tumours, urgent distension from flatus, &c. A few cases of its use in retention of urine have also been recorded—but very few. Dr. Bell proceeds to give the details of a case which he believes to be the first in which this instrument has been used for this purpose in Scotland. The patient, aged 65, was admitted under Dr. Bell's care on the 18th June, 1873. He was a feeble, puffy old man; was in a semi-comatose state, but could be roused, and could answer questions. His eyelids were swollen, and his limbs cedematous. He had a weak, rapid pulse and a harassing cough, with the physical signs of bronchitis and emphysema. His scrotum and penis were enormously swollen, and of a dark red colour. There were several sinuses in the scrotum and perineum. The perineum was very thick and brawny. He had passed no urine by the urethra for three days, but a few drops had dribbled away by the sinuses in the perineum. His bladder could be felt distended nearly up to the umbilicus. He had, in addition, a reducible scrotal hernia on the right side, which rendered the urethra tortuous. He had been cut by Mr. Syme fourteen years previously, and Mr. Bell had himself dilated his stricture to full size five years previous to his admission; but since then, until six months before admission, he had no treatment at all, and his stricture had become much worse. Mr. Bell at once tried to pass small instruments, and found that a stricture existed about two inches from the orifice, which yielded readily to dilatation; but behind this there was a track of tight urethra coated with phosphatic salts, and densely hard. In the condition the patient was, it was not considered prudent to perform perineal section, so No. 2 needle of Dieulafoy's aspirator was inserted in the middle line about  $1\frac{1}{2}$  inches above the symphysis pubis, and by it above a pint of fetid, turbid urine, of specific gravity 1017, was drawn off. The urine contained a large quantity of albumen. The needle was at once withdrawn, and the patient was placed in a warm bath. Great relief followed. In a few hours the patient began to be able to get some urine passed. On the third day a No. 2 elastic bougie



was passed into the bladder, and in a short time the urethra was dilated to admit No. 11. Several small phosphatic concretions were passed, and the patient regained strength. No trace of the puncture was visible after the first few days, nor did it occasion the slightest inconvenience. In fact the patient voluntarily remarked that it was the easiest way of having the water drawn off he had ever experienced. He made a good recovery, and left the hospital with a urethra admitting No. 12. Perineal section is always tedious, requiring chloroform, and has its own dangers in old, exhausted subjects. Tapping by rectum would have been difficult, from the enlarged prostate. Catheterisation had failed. The aspirator appears to supply an easy, safe, and reliable means of tiding over a difficulty, emptying the bladder and thus giving time for other treatment. (*Edin. Med. and Surg. Journal*, April 1874.)

### Removal of Hæmorrhoidal and other Tumours.—

Mr. Henry Lee observes that at one time Dupuytren, Cline, and Brodie were in the habit of removing internal piles by the scissors; but that in some instances the patients nearly bled to death, and in subsequent cases the ligature was used. This, however, often caused acute pain, and in the method formerly adopted of transfixing with a needle and applying a ligature on the proximal side danger was not always avoided, as the chief vein was sometimes transfixed; and Mr. Lee has himself made examination where secondary deposits were found in the liver, and nowhere else, though the operation had been skilfully performed by a practised surgeon. Patients used to complain of the time they were kept in bed. Recent experience has taught Mr. Lee that the operation may be performed with safety, and without dragging upon the parts (a fertile source of pain), by the elastic ligature. He seizes the pile with a pair of forceps, which closes with a spring or a screw, and terminates with a ring. The elastic ligature is carried with the point of the finger several times round the end of the forceps and tied. The protruding pile or piles are then returned into the rectum. They slough off in the same manner as if tied with a silk ligature; but there is this additional security, namely, that the ligature always remains tight. The elastic ligature, if used for this purpose, should be solid. If tubular, when stretched it flattens against the parts, and consequently does not cut through them so easily. By far the best and safest way, however, according to his experience, of removing a pile, is to grasp it at its base with a clamp made in the shape of a pair of scissors curved on the flat, and with the points turned inwards; then to cut the pile off with a pair of scissors of the same shape, and to sear the cut surface with an iron after it has passed from a dull red to a black heat. When

the mucous membrane and the parts below are not much thickened, a very great deal may be done by constitutional treatment—ablution with cold water, and especially by the local use of suppositories containing the powder of cubebs. In cases, again, where these fail, the application of the strong nitric acid to the tumour made to project through a hole in a glass speculum is an excellent remedy. (*Lancet*, April 18, 1874.)

**Successful case of Abdominal Section for Intussusception.**—Mr. Hutchinson communicated to the Medico-Chirurgical Society a successful case of abdominal section for intussusception in a child aged two years, and added notes of a considerable number of cases bearing upon the diagnosis and treatment of similar cases, from which he drew the following conclusions:—1. That it is by no means uncommon for intussusception to begin at the ileo-cæcal valve, and to progress to such a length that the invaginated part is within reach from the anal orifice, or even extruded. 2. That it is of great importance in all cases of intussusception to examine carefully by the anus. 3. That in almost all cases of intussusception in children, and probably in most of those in adults, the diagnosis may be made certain by handling the invaginated part through the abdominal wall. 4. That the prognosis of cases of intussusception varies much; first in ratio with the age of the patient, and secondly with the tightness of the constriction. 5. That in a large proportion of the cases in which children under one year are the patients, death must be expected within from one to four or six days from the commencement. 6. That in the fatal cases death is usually caused by shock, or by collapse from irritation, and not by peritonitis. 7. That in many cases it is easy, by estimating the severity of the symptoms (vomiting, constipation, &c.), to form an opinion as to whether the intestine is strangulated or simply irreducible. 8. That in cases of strangulated intussusception, whilst there is great risk of speedy death, there is also some hope that gangrene may be produced, and spontaneous cure result. 9. That in cases in which the intussuscepted part is incarcerated and not strangulated, there is very little hope of the occurrence of gangrene, and it is probable that the patient will, after some weeks or months, die, worn out by irritation and pain. 10. That the chances of successful treatment, whether by the use of bougies or by injection of air or water, are exceedingly small, except in quite recent cases; and that if the surgeon does not succeed by them promptly, it is not likely that he will succeed at all. 11. That the cases best suited for operation are those which have persisted for some considerable time, and in which the intestine is only incarcerated; and that these cases are also precisely those least likely to be

relieved by another method. 12. That in the cases just referred to, after failure by injections, bougies, &c., an operation is strongly to be recommended. 13. That the records of post-mortems justify the belief that, in a considerable number of cases referred to, the surgeon will encounter no material difficulty after opening the abdomen. 14. That the circumstances which might cause difficulty are (*a*) the tightness of the impaction of the parts; (*b*) the existence of adhesions; and (*c*) the presence of gangrene. 15. That in selecting cases suitable for operation, the surgeon should be guided by the severity of the symptoms to an estimate of the tightness of the strangulation, and as to the probability of gangrene having already set in. 16. That in cases in which the patient's symptoms are very severe, or the stage greatly advanced, it may be wiser to decline the operation and trust to the use of opiates. 17. That the operation is best performed by an incision in the median line below the umbilicus. 18. That in cases of intussusception in young infants (under one year of age) the prognosis is very desperate, scarcely any recovering except the few in whom injection treatment is immediately successful, whilst a large majority die very quickly. 19. That the fact just mentioned may be held to justify, in the case of young infants, very early resort to the operation. 20. That it is very desirable that all who, in the future, have the opportunity for post-mortem examination of intussusception cases, should give special attention to the question as to whether an operation would have been practicable, and should record their results. (*Proceedings of the Royal Medical and Chirurgical Society of London*, vol. vii., No. 5.)

**Freckles.**—For the benefit of young persons afflicted with freckles, it may be stated that powdered nitre moistened with water, applied to the face night and morning, will soon remove all traces of them. (*The Druggist*, February 1874.)

## Extracts from British and Foreign Journals.

**Suggestions for securing greater precision in reporting Cataract Operations and Results.**—Dr. Green, of St. Louis, remarks that it is important in reporting and tabulating cataract operations that in each case the exact form, extent, and position of the corneal section should be noted; also the character and iridectomy, if iridectomy have been performed, the manner of opening the capsule or otherwise dealing with it, and the measures adopted for facilitating the exit of the lens. A detailed record should also be kept of any complication or defect in the completed operation. And in the immediate result of the healing process, and in estimating the final result, we should, in addition to giving the measure of the acuteness of vision, be careful also to detect and note any irregularity in the healing of the corneal wound, any synechia or other irregularity of the pupil, any remains of capsule or other pupillary obstruction, and any abnormal condition of the vitreous or of the fundus, as shown by the ophthalmoscope. The record of the form, extent, and position of the corneal section, the character of the iridectomy, and the ultimate form and condition of the pupil, may be illustrated by simple diagrams, with the very great advantage of showing these details of the operation and result as they have appeared to the eye of the operator, and so supplementing and correcting defects and inaccuracies of statement. Such diagrams are best drawn upon engraved blank forms, of which Dr. Green gives a series of specimens. These may be printed in any light colour, and gummed on the back like a postage-stamp, for insertion in the case-book. The forms which he recommends are three in number, namely, a front and a profile view of the cornea, and a front view of the iris. (*Transactions of the American Ophthalmological Society. Ninth Annual Meeting.*)

**Local treatment of Vomicæ.**—Dr. Mosler states that in two hopeless cases of phthisis, where large cavities existed, he adopted the plan he had already tried in pleurisy, of puncturing



through the skin and injecting a solution of permanganate of potash, and found that not only were no injurious effects in the form of inflammation excited, but that some improvement in the general health was experienced. Encouraged by these results, he determined to make a direct fistulous passage into a large vomica that existed in the right lung of a man aged 49, who had been ailing for five years, and who was also suffering from amyloid degeneration of the kidneys and intestines. The fistulous passage was made in the second intercostal space about an inch and a half from the right border of the sternum, on the 2nd July, 1873; the soft parts being cut through and a puncture made through the pleural laminae, through which a silver canula was passed. No febrile symptoms followed the operation, but an abundant discharge of pus took place through the canula, especially on coughing, and the general health immediately underwent improvement. On the occasion of some hæmoptysis a little dilute solution of perchloride of iron was injected, or rather allowed to be inhaled through the canula, with the effect of arresting the flow of blood. Subsequently the patient was directed to inhale pulverised solution of carbolic acid and tincture of sodium in the same manner. The pus became laudable and diminished in quantity, and the degenerative process in the lung appeared to be making no further progress. Nevertheless the patient became gradually more and more feeble, and finally died four months after the operation. Pus continued to be discharged up to the time of death, and carbolic acid solution was thrown in in the pulverised form twice daily. On post-mortem examination it was found that both pleural laminae had become firmly adherent to each other, and at the upper part they formed a thick whitish pseudo-membrane of almost cartilaginous consistence. The fistulous passage was found to open into a large cavity occupying almost the whole of the upper lobe of the right lung, which contained a yellowish creamy fluid. The interior of the cavity was lined by a false membrane, presenting here and there processes the edges of which were beset with granulations. The spleen, kidneys, and intestines were affected with amyloid degeneration. From experience of this case Dr. Mosler arrives at the conclusion that the lung-tissue is tolerant of external agents, which may be applied more freely than has hitherto been considered possible, and that this method of treatment is decidedly worth further trial. (*Berliner Klin. Wochenschrift*, 1873, No. 43.)

**Additional Notes on Pancreatin.**—Dr. Richard V. Mattison remarks that numerous inquiries regarding saccharated pancreatin, its doses, uses, &c., having been instituted since the publication of his last paper upon the subject, induce him to



present a few further remarks upon this valuable and highly interesting substance. It is a fine white powder, almost tasteless, or with the slightly sweet taste of lactic acid. When mixed with water it is perfectly soluble, dissolving in between five and six parts of that liquid, and forming a perfect emulsion when mixed with liquid fats and a small quantity of water.

He suggests the following formula for the proper exhibition of cod-liver oil in combination with pancreatin:—

R. Pancreatini sacchar. ʒj  
 Aquæ fʒiv  
 Sacchari albi ʒvij  
 Olei morrhuae Oiss  
 „ Gaultheriæ gtt. xx  
 „ Amygd. amar. gtt. v  
 M.

Rub the saccharated pancreatin with the sugar and water, in a mortar, until a thick syrup is formed; to this add the cod-liver oil, in which the essential oils have been dissolved. This forms a perfect emulsion, without difficulty. It separates, of course, upon standing, but can easily be shaken together again, forming an emulsion with slight agitation. This is certainly to be preferred to the thick mucilaginous emulsions made with gum arabic or tragacanth, which are usually so distasteful to patients, because of their being so thick.

By the above formula, a preparation can be furnished containing 75 per cent. of oil, in the condition in which the oil, as usually prescribed, enters the duodenum, thus rendering its absorption and assimilation by the lacteals comparatively easy, the molecular formation of the oil being completely broken up. To make the emulsion whiter a little lime-water may be substituted, omitting an equivalent quantity of water, a partial saponification rendering the emulsion more permanent and more elegant in appearance.

The easy assimilation of this preparation having been experimentally demonstrated by several of our eminent medical practitioners, it stands unrivalled in a therapeutical point of view as a standard pharmaceutical preparation of cod-liver oil.

The action of pancreatin upon albumen having frequently been stated, experiments were instituted in order to more closely examine this action. Accordingly, ten grains of saccharated pancreatin were dissolved in one fluid ounce of water, with the addition of six drops of hydrochloric acid. To this thirty grains of coagulated albumen were added, and the whole kept at a temperature of 100° F., being occasionally agitated. At the end of six hours about twenty-five grains were dissolved, thus showing the correctness of the usually received statement, and

at the same time showing its great inferiority to saccharated pepsin, which under similar circumstances would have dissolved from 120 to 180 grains.

The action of pancreatin upon starch was next observed, and a drachm of Bermuda arrowroot was mixed with a solution of ten grains of saccharated pancreatin in one fluid ounce of water, and kept at the temperature of 100° F. for several hours. At the expiration of this time the mixture was filtered, and the filtrate tested for glucose, abundant evidence of the presence of this substance being afforded by Trommer's and Fehling's tests. That this glucose was the product of the action of pancreatin upon starch was demonstrated by testing the arrowroot, which gave a negative result, and by a comparative test with the solution of glucose obtained by the above action of the pancreatin upon the arrowroot. Fifty minims of this solution, containing less than one grain of lactic acid, reduced the cupric solution much more readily than a similar solution containing ten grains of lactic acid, the purity of both specimens of milk sugar having been ascertained before using.

Although the strength of saccharated pancreatin is given as "ten grains emulsify two fluid drachms of cod-liver oil," yet when ten grains were dissolved in one fluid drachm of water, and one fluid ounce of oil added, perfect emulsification was effected in a very few minutes. (*Pharmaceutical Journal*, February 21, 1874.)

**Oil of Turpentine as an Antidote in poisoning with Phosphorus.**—Dr. Herman Kochler, in a paper on the value and significance of the oil of turpentine containing oxygen in phosphorus poisoning, sums up his results as follows:—Oil of turpentine containing oxygen is a reliable antidote of phosphorus. The tree from which the turpentine is derived, whether German or French pine, is of no importance, if it has only not been rectified for some time and contains oxygen. The oil of turpentine *only* acts if introduced into the stomach, best by gelatine capsules; to emulsify the turpentine with the yolk of an egg is not advisable, as the latter is oleaginous and therefore dissolves the phosphorus. For every 1.5 grains of phosphorus about two drachms of turpentine must be administered, and this should be given as soon as possible after the poisoning; the oil of turpentine being of no service twenty-four hours after the introduction of the phosphorus. The beneficial action of the antidote is due partly to its containing free oxygen for changing the poisonous phosphorus to the non-poisonous phosphoric acid, and partly by combining with the phosphorus to form the turpentine-phosphorus acid, which is not injurious. This acid, which is only dangerous in very large doses, appears (like oil of turpentine and camphor)

to be eliminated unchanged by the kidneys. It and its salts rapidly absorb oxygen from the air and oxidise to a resinous derivative of the oil of turpentine, containing phosphoric acid, and this rapid oxidation has prevented an analysis for the ultimate elements. (*New Remedies*, vol. iii., No. 2.)

**The Ailanthus Glandulosa in the treatment of Dysentery.**—M. Robert, the principal medical officer attached to the naval division of China and Japan, gives an account of the remarkable results he has obtained in the treatment of dysentery by the *Ailanthus glandulosa*. The plant belongs to the Xanthoxylaceæ, is widely spread in China, is sufficiently common in our gardens, and is easily recognisable by its long pinnate leaves, remotely resembling those of the ash. M. Robert has found that in hot climates it gives results superior to those of ipecacuanha, calomel, and astringents, either with or without opium, and to the treatment by milk. The bark of the root is the only part used, and the mode of preparation is to make an infusion of about two ounces by weight in four ounces of water. The bark is lightly bruised, and the infusion is "passed through a filter. The dose is a dessert-spoonful morning and evening, either by itself or in a cup of tea, for three days, great attention being at the same time paid to the diet. If at the expiration of eight days the affection is not cured, the treatment should be recommenced. The infusion is very bitter and produces nausea, and if the above dose is doubled vomiting is induced. In four of Dr. Robert's cases the ailanthus was given, with other remedies; in twelve others it was given alone, and a cure was always effected in from eight to twelve days. (*Archives de Médecine Navale*, Feb. 1874.)

**Rest as a Therapeutic Agent in the treatment of Disease.**—Dr. Finny, of Dublin, in a thesis for his degree, points out that rest, as a curative means, is more generally recognised and adopted in surgery than in medicine, in which it is of no less importance. Rest and sleep, however, do not of themselves restore the power to the weary limbs or vigour to the flagging spirits; they do but place the individual in the best possible position for Nature's recuperative powers to exercise their influence. Rest is not only to be regarded as a means of repair, but it is prophylactic in its nature, and as such is constantly acted on and generally recognised as an integral part of success in life, whether in the frame of man or of his beasts of burden. In the treatment of disease, rest may be induced in various modes. Thus many cases might be adduced, and Dr. Finny cites one of abdominal cancer, in which great improvement followed the hypodermic injection of morphia. Another was reported by Dr. Stokes, in which a powerful man, suffering from

an extremely violent attack of smallpox, was at once relieved by placing him with pillows to support his head in a warm bath, in which he was kept for seven hours, during which time brandy was freely given as a restorative, and only omitted when he showed symptoms of the brain being affected by it. The surface became clean, and in many places the sores looked healthy and white; the bath was repeated next day, after which he fell for the first time into a tranquil slumber and steadily improved from this period. Again, over-exertion of the brain or mind is by no means an uncommon cause of delay in the convalescence from fever amongst the better classes, as too often the interest of some entertaining book overrides the patient's prudence, and thus a state of sleeplessness and nervous excitability is readily produced. Remove the exciting cause and soothe the condition by some nerve sedative, such as the bromides of ammonium or potassium, and how rapid the change, how progressively the improvement will continue!

In the consideration of rest in relation to the circulatory system, Dr. Finny alludes to its application in inflammation of the pericardium. The treatment of this disease, whether it occur as a concomitant of rheumatism or of other affections, by antiphlogistic remedies and lowering diet, is now, he trusts, a thing of the past, whilst the treatment by opium, urged by Stokes, is that which is almost universally adopted. The drug acts here not as a soporific, as the large and frequently repeated doses which are tolerated by patients suffering from this affection show; but its influence is seen by quieting the action of the heart, relieving pain, and checking over-excitement of the organ; and thus, by giving physiological and natural rest, it becomes a therapeutic agent unrivalled in the universality of its application and in the certainty of its results. In the treatment of serous effusion into the pericardium, the principle of rest is not less important, for should it not be obtained, the feeble powers of the heart will fall still lower, and death result. Here opium cannot be used, but efforts must be made to effect the absorption of the effused products which hamper the free action of the ventricles, by calling into action the skin, bowels, and kidneys, or if these prove ineffective, the fluid must be removed mechanically. Rest is equally beneficial in cases of organic disease affecting the walls or valves of the heart. Dr. Finny then gives a series of cases of different forms of disease of the heart and lungs, in one of which venesection, in another digitalis, in another mercury, in another chloroform inhalation, and in another the recumbent position, with venesection and special diet, were the means by which rest was procured and recovery effected. (*Dublin Journal of Medical Science*, Feb. 1874.)



**Guaiacum in the treatment of Syphilis.**—Dr. Alex. McBride, in a letter addressed to the *Cincinnati Lancet and Observer*, *à propos* of a discussion in their Academy of Medicine, observes that many practitioners give guaiacum the go-by, and think it belongs to old rubbish; but that he has had large experience with it, and he thinks the chief reason it has fallen into desuetude is that it is so hard to get into the stomach in sufficient doses. In the case of the decoction large doses require to be taken for months. But this is not so bad as the tincture; for however made up, it is repulsive, and soon becomes intolerable. It should then be made up into pills, which will be taken readily enough; but the success of the pills depends upon how they are made. Alcohol is the only proper excipient. The resin should be powdered, and ligneous and cortical impurities be sifted out; the mortar, pestle, and pill-machine should be warm, and a very little alcohol added; it can then be made up quickly into pills, which will keep well. Of these about a dozen can be taken daily, and they answer well for secondary and tertiary manifestations (especially cutaneous) of syphilis. (*Cincinnati Lancet and Observer*, vol. xxxiv. p. 761.)

**Stricture of the Urethra.**—Dr. Leopold Dittel commences an article on strictures of the urethra in *Pitha and Billroth's Handbuch der Chirurgie*, by an account of the anatomy of the urethra and of the bladder, and a discussion of the mode of introducing the catheter. The muscular layers which he admits to be present in the muscular portion or isthmus of the urethra are—1. The organic muscular fasciculi contained in the pubo-prostatic ligament. 2. Those contained in the so-called ischio-prostatic ligament. 3. The striated musculus transversus peronei profundus, which is situated close behind the lamina media, and extends from one tuber ischii to the other, enclosing Cowper's glands. 4. The circularly disposed musculus compressor urethra. Dittel calls Amussat's valvula pylorica the organic sphincter internus, since its atrophy is frequently followed by incontinence of urine. He considers that the cases in which a catheter may be retained in the bladder are—1. Where there are large false passages. 2. When the urethra has suffered from blows and contusions. 3. In cases of paralysis of the bladder; and 4. In cases of hypertrophy of the prostate with retention of urine, especially if the introduction of the catheter is attended with great difficulty. The choice of the material of which the catheter is composed in cases where it is requisite that it should be retained in the bladder, is dependent on various circumstances, though any catheter may, on occasion, be employed. On the whole, he appears to think that Holt's hinged catheter is the simplest and safest construction. He divides



strictures into the spasmodic, inflammatory, and organic. In the treatment of spasmodic strictures, Prof. Dittel prefers to place the patient in a warm bath, and to give narcotics, lupulin, &c., prior to making any attempt to introduce the catheter. In inflammatory strictures he adopts active antiphlogistic treatment, leeches to the perineum, narcotics, yellow ointment, hot Sitz-baths, and also the application of cold, whilst attention should be paid to the peculiarities of each patient. If after a gonorrhœa the patient continues to have a burning sensation in the urethra, caused by a swelling of a part of the urethra, or if some part of the urethra is painful, owing to the unskilful introduction of the sound or bougie, Dittel recommends—1. Rest, and avoidance of all sexual excitement. 2. Cold Sitz-baths and poultices. 3. Pressure by means of heavy metal sounds; and, if this does not prove successful—4. Cauterisation with his *porte-remède*. Cauterisation by means of Dittel's *porte-remède* is effected not by the caustic in substance, but by thin rods of cacao-butter, about an inch in length, and weighing from four to five grains, containing from  $\frac{1}{2}$  to  $\frac{1}{3}$  of a grain of nitrate of silver. The action is very energetic and persistent. Dittel divides the organic strictures in the following manner:—

First Order. Neoplastic connective-tissue formation.	{	I. Parenchymatous	{	First kind, Callous.	{ Diffuse. Knotted. Annular.
				Second kind, Atrophic.	{ Marastic. Traumatic. Ulcerative, Cicatricial.
Second Order. Heteroplastic new formations . . .	{	II. Free . . . . .	{		Caruncle. Valvular. Cord.
					Epithelioma. Sarcoma. Collonema. Tubercle. Fibro-carcinoma.

In regard to treatment, Dittel divides it into that by dilatation, that by cauterisation, and that by urethrotomy. The various methods of dilatation are described by Dittel as—(1) Temporary and gradual, (2) permanent and gradual, and (3) permanent and forcible. The instruments he uses are the usual catgut bougies, gum elastic bougies, metal sounds, and dilators. He maintains that the catgut bougies are the most irritating to the urethra, and he only uses them in the tightest strictures. As soon as he succeeds in introducing one of the size of No. 6 of Charrière's scale, he exchanges it for a conical silver catheter. He alludes to Benigné's plan of introducing several catgut bougies at the same time, and also to Tedeski's sounds, made of hardened tin. The time that a sound should be allowed to remain in the urethra cannot be determined beforehand. Some patients can endure it

for months without suffering, whilst others cannot bear it for more than a few minutes. Catgut bougies should not be allowed to remain for more than ten minutes. Atrophic strictures are peculiar in the circumstance that they become tighter and embrace the bougie more firmly in the course of ten minutes or a quarter of an hour, and then render the extraction more painful for the patient than its introduction. The converse is the case in callous strictures. Dittel prefers the metal sounds in almost all cases, and recommends them for self-catheterism. The circumstances rendering dilatation difficult, in addition to the position of the stricture at the isthmus, are—

1. Fistular passages which deflect the urethra from its ordinary course, and render the use of metal sounds inappropriate. In such cases the dilatation should be commenced with elastic bougies, and if these prove a failure, an incision should be made from without.
2. Large callous masses, which are firmly connected with the symphysis, form an insuperable obstacle to the introduction of stiff sounds. Here soft and yielding bougies should be employed, and efforts made to promote the absorption of the new formation.
3. The dilatation is sometimes so painful that the patient is unable to endure it. This is especially the case in strictures of the glans and external orifice of the meatus; and in such cases Dittel recommends either incision or the introduction of conical sounds. The unpleasant circumstances which may follow dilatation are—

1. Swelling of the stricture and retention of urine.
2. Bleeding and false passage, which last is particularly dangerous when it has been made between two strictures. The urine passes through the posterior stricture, but it is prevented from passing forward by the anterior one, and enters the false passage, causing infiltration of urine, gangrene, and septicæmia.
3. Traumatic bleorrhœa, causing the pressure and friction of the sound against the urethra, epididymitis, cavernitis, periurethritis, cystitis, and acute pyelitis are also amongst the sequelæ.
4. Prostration or reaction attended with lowness of spirits and exhaustion; and
5. A peculiar affection—urethral fever—caused by the reaction of the urinary organs and the sympathy of the system generally with the local lesion. Of this he distinguishes three forms:—

1. The nervous form with pure reflex reaction.
2. Fever caused by lesion of the urinary tube; and
3. The so-called morbid form of urethral fever, which is associated with disease of the kidneys and their pelves, and often ends fatally. (*Handbuch der Chirurgie v. Pitha u. Billroth*, Band iii. Abtheil. ii., and *Der Praktische Arzt*, Band xv. Heft i. 1874.)

**Treatment of Diphtheria.**—In a clinical lecture by M. Buequoy, delivered at the Hôpital Cochin, after describing

the symptoms of the disease, its form and diagnosis, observes that Bretonneau first pointed out the importance of active treatment for its cure, and adopted the plan of cauterisation with hydrochloric acid. Trousseau subsequently employed nitrate of silver. M. Bucquoy is opposed to the use of violent caustics, and is of opinion that the false membranes should first be removed by forceps, and that then the raw surface should be acted on. Lotions of alun, lime-water, chlorate of potash, perchloride of iron, carbolic acid, tannin, and nitrate of silver, may be applied, but he thinks lemon-juice is preferable to them all, and it may be used by dipping a little plug of cotton-wool twisted round a knitting needle, and pressing it against the surface four or five times a day. On the first appearance of the disease, especially in the lighter forms, emetics should be prescribed once or twice; and chlorate of potash is very serviceable in cases of moderate severity. But when the attack is very violent, recourse must be had to wine, brandy, coffee, and food. Blisters should never be used. (*Journal de Médecine*, March 1874.)

**Treatment of Trichiasis.**—M. Menu describes a new operation for the relief of trichiasis, which consists in making an incision parallel to the border of the lid, at some distance from its edge, and through the whole thickness of the skin. The fasciculi of the orbicularis exposed on reflecting the integument upwards are excised, and the tarsal cartilage is laid bare. The flap containing the roots of the cilia is now dissected back as far as to the edge of the lid which is not injured. A thread is passed by means of a curved needle through the suspensory ligament of the tarsus, then under the inferior flap as far as to the free border of the lid, at which point it issues on the level of the cilia, and the loop is tied. This is done at several points, and the ends of the threads are brought up over the forehead, and fixed by a strip of plaster. (*Pamphlet*.)

[OUR CORRESPONDENCE and BIBLIOGRAPHY are unavoidably delayed till next month.]

## Department of Public Health.

### THE PROSPECTS OF PUBLIC HEALTH.

BY H. W. RUMSEY, M.D.

ONE thing only seems clear in the misty chaos of opinion which now perplexes and baffles the advanced body of the sanitary party—namely, that there is no efficient machinery for the administration of public-health law.

We may differ as to the causes of recent disorganisation and consequent failure of effort ; but the fact is indisputable. Would it not therefore be well, before further attempts are made at legislation, to inquire more closely than heretofore, yet candidly and disinterestedly, into those causes ? But thus to inquire, obliges us to look into the previous state of affairs, to examine the steps which have been taken of late years, whether by the Legislature or by the Government for the time being, to rectify notorious anomalies and imperfections.

The administration of public health and of local government as far as health is concerned, rested upon three comparatively old systems : (1) the Privy Council, acting by Royal mandate, in the prevention of disease and public vaccination ; (2) the Poor Law Board, acting for the ratepaying interest, in the relief of destitution ; (3) the Home Office, acting for Parliament through two subordinate departments, the Local Government Act Office in engineering matters, and the Registrar-General in the record and publication of vital statistics. The Registration sub-department, from its commencement, had established, locally, an administrative connection with that for destitution, utilising

its areas and authorities of local administration; so, more recently, had the Privy Council, in its proceedings concerning disease-prevention and vaccination, and this mainly because the Poor-law department had first of all grasped the administration of *medical* poor relief. On the whole the complication was most singular, very difficult to unravel and reduce to order and reason.

It was hoped by the memorialists who procured the appointment of a Royal Sanitary Commission, that the Commissioners would have laid down some rational principles of co-operation and mutual arrangement, based on sound theory, and so leading to correct practice. But the belief of our continental neighbours, that the English have no faculty for organisation, seems to have been verified by the legislative result of that Commission, for the able men who formed it came to no positive or satisfactory conclusions as to the precise sanitary use which should be made either of the existing Government departments or of the established local bodies.

As to the former, the Report of the Commission refers throughout to a somewhat undefined "Central authority," although it distinctly lays down (pp. 30-32) the conditions on which the future department for sanitary administration might be combined with the existing department for the relief of destitution.

We wish to give the Commission full credit for having plainly protested against the possible subordination of "the care of the public health to the provision for infirmity and . . . destitution,"—and for having as plainly indicated a "central authority, which should preside over sanitary administration as one sub-department, and over Poor-law administration as another sub-department." For "the sanitary department was to be as universal and equal in its action throughout the kingdom . . . as its co-department of the Poor Law."

Strange, however, as it may seem, the late Government brought in the measure of 1871, constituting a Local Government Board, without the smallest provision to secure this equal co-ordination of the two departments. The new Board was, by the Act, to consist of the same persons as constituted the old Poor Law Board—not a single member was added to represent sanitary



knowledge or sanitary administration. Although the Lord President of Council and the Secretaries of State, as representing former Health departments, were named, they were previously members of the Poor Law Board, as were also those ornamental appendages, the Lord Privy Seal and the Chancellor of the Exchequer. It was, from the first, a mere destitution authority, changed only in name, and offering not the slightest guarantee to the defenders of the public-health cause for the requisite change of policy. The well-known traditions, opposed alike to medicine and to hygiene, of the old Poor Law Commissioners, might well have warned all medical and sanitary philanthropists against such an inauguration of the new Act. But the warning was disregarded.

The somewhat hazy idea of correct central administration presented by the Royal Commission was from the first lost sight of by the framers and administrators of the Act; and the first President of the Local Government Board entered upon office, not only with the old *ex-officio* surroundings, but even with the old discredited staff. Yet, among sanitary reformers, Mr. Stansfeld had strong personal friends, who, regardless of the proclivities of his satellites, hailed his advent to power, and succeeded in driving a miserable Act through Parliament, in opposition to the protests of the most experienced sanitarians.

We now see the lamentable result. The chief medical adviser of the former Privy Council department, who may truly be called the founder of official scientific inquiry into all matters affecting the public health, and to whom the country lies under large obligations, has been virtually set aside. His reports are pushed into comparative obscurity. The valuable scientific inquiries on which his reports are based were at once compressed into the briefest possible *précis* in almost illegibly small type. His function of supervision and direction, through inspectors, has not been made "universal and equal in its action throughout the kingdom with its co-department of the Poor Law." (See Report of Sanitary Commission.)

Not content with this unwarrantable depression of the medical department, some anonymous legal traducer, writing in the interest of the old poor-law staff,<sup>1</sup> has endeavoured to

<sup>1</sup> See "Lex," in the *Daily News*.

damage still further Mr. Simon's position, and if possible to get rid of him as an integral part of central sanitary administration, by an entire misrepresentation of his work, for which that writer could not have the excuse of ignorance.

But, obviously, the main cause of the failure of the Public Health Act is to be found in the organisation of local authorities and local officers, for which the Sanitary Commission is quite as responsible as either the Act itself or the central authority which it created. In fact, the local authorities, constituted in a very defective and objectionable manner, and acting in very badly contrived districts, without concert or regulation, are fully empowered, each for itself, to set aside every recommendation of the central authority, with respect to the correction of district boundaries, the unification or conjoint action of authorities, and the appointment of officers.

Notwithstanding some very sensible remarks (in the Report of the Commission) on the causes of imperfection in sanitary administration, among which causes the Commissioners plainly include "confusion of authorities" and "variety of areas,"—specifying under the latter "want of coincidence of the several areas of various jurisdictions" (p. 21); notwithstanding their acknowledgment that the different aggregates of parishes that had been made under former enactments, whether for destitution-relief, or for highway management, or for magisterial action in petty session, had led to all kinds of administrative perplexities; and notwithstanding their assertion that the want of uniformity between the areas for municipal and sanitary government, and the frequent subdivision of parishes under two or more jurisdictions for similar objects" (p. 20), render it impossible that there should be either "combined efficiency, or general economy,"—notwithstanding all these admissions, the Commissioners, with strange inconsistency, allowed this confusion of areas to remain; nay, they established it more firmly by conferring fresh powers on the same unreformed authorities in the same uncorrected districts.

Since petty sessional districts and highway districts depend on county authority, and, like municipal districts, have no conformity or correlation with registration districts or with medical relief districts,—it was naturally expected that the first step

would be to reconcile union with county and borough boundaries. But nothing of the kind was recommended. Yet the Commissioners had no excuse for this omission, for they were well aware that the county, like the borough, exercised many distinct functions appertaining to the public health.

There was less ground for disappointment at the fact that the omission was not supplied by Mr. Stansfeld, whose antecedents might lead him naturally to distrust the old county authority; but he soon awoke to the necessity for some organic change in the limits of the various conflicting jurisdictions; so he moved in April 1873 for the appointment of a

“Select Committee of the House of Commons to inquire and report whether the existing areas and boundaries of parishes, unions, and counties might be so altered and adjusted as to prevent the inconvenience in matters of local administration and taxation which now arises from the limited extent or subdivision of certain parishes, or the overlapping of parishes in two or more administrative areas, or from parishes and unions being situated in more than one county, with power to recommend whether any, and if so what, measures should be taken to give effect to their Report.”

Why, then, did not some fundamental reform of organisation result from this inquiry? Dr. Farr's evidence alone is sufficient to determine its necessity; and great disappointment was reasonably felt that no Boundary Commission followed upon that Report. Will Mr. Selater-Booth, aided as he would be by the county party, at once inaugurate a true reform in local government, by adjusting the areas in which it is administered?

The Sanitary Commission reported, p. 53—

“Unions, and sometimes even parishes, overlapping county boundaries; registration districts making incomplete correspondence with them in statistics of births and deaths; highway districts made optionally and irrespectively of all other areas, or coinciding sometimes with one, sometimes with another; petty sessional divisions generally differing from all—cause altogether to a country whose life is self-administration, probably the maximum of embarrassment and waste of local government, and the utmost loss of means and effectiveness. The same boundaries should as far as possible define the areas of all these kinds of provincial executive, and their officers should be, as far as possible, the same for all those purposes.”

At the same time, as they truly say, p. 54—

“Areas for one purpose must be larger than areas for another; areas for registration or river conservancy, for instance, must include several smaller, but the unit of area should be the same for all local purposes, and larger areas should be as far as possible exact multiples or aggregates of that unit. That even now, far

as we have wandered from this essential condition of efficiency, the confusion of administration may be unravelled and retrieved, is proved by our evidence from Hampshire."

Nor need urban districts form any exception to a general adjustment. If the town councils and local boards (omitting those of the largest centres of population as wholly independent of surrounding governments) were properly represented in the conjoint boards, which would naturally result from a rectification of boundaries and a consolidation of arrangements for taxation and local management, every unit of administration might retain its just right of self-government, so far as that consists with the general welfare.

A second and scarcely subordinate cause of recent failure in urban sanitary administration, is the continued attempt to keep apart and distinct the local machinery for destitution—in so far at least as its medical and preventive duties are concerned—from the local machinery for health. This is the more unreasonable and indefensible, because the consolidation of central authority effected by the Act of 1871 established a contrary principle. If the Poor Law Board was, or might have been made, a proper *central* authority for public health, the local boards for relief of destitution might also have been constituted, with the addition of other elements, the *local* authorities for health. Even now, after the areas of local administration are adjusted, it will be no difficult matter to constitute such bodies for the management of all matters relating to both health and disease, and so to put an end to the needless and absurd differences now generally prevailing between Boards of Guardians and "urban" authorities on these questions.

The argument of the Commissioners (p. 32), as to the cognate character of provisions for the public health and for the relief of destitution, applies as well to local government as to central.

The sub-department of Town Improvement, as a distinct subject, might be combined with the other sub-department of relief of destitution and prevention of disease, by means of a joint representative Committee or Council of Health. Mr. Simon was fully aware of this when, with prescient sagacity, he referred to this subject in his 11th Annual Report.

Assuming that in urban districts the health authority might



for the present continue to be distinct from the destitution authority of the place, he said :—

“ I accept this as a fact, but I doubt if it is to be deemed an advantage. I suspect that it would generally be of benefit to municipal administration if one single board, with sufficient power of acting by committee, were elected for all purposes of local government, including the relief of destitution.” (P. 26.)

On the existing basis of legislation, the solution of the question seems to depend, in the first place, in dealing with both the health and the sickness of the masses by a single authority in each place, an authority formed by delegates from the existing boards and councils.

The same solution might, indeed, be effected by separating medical relief, vaccination, and medical preventive action of every kind, from the local relief of destitution under the Poor Law, and combining them with local sanitary management. Mr. Simon referred to this plan in his answer about workhouse lunatics (2,139) before the Sanitary Commission. He said :—

“ That rather forms part of the very difficult question whether medical relief altogether ought to go with the relief of destitution or not. I confess I do not see my way to separating them, although I know it to have been very strongly urged before Parliamentary Committees and elsewhere that a separation should be made.”

If this separation be really impracticable, there is nothing to fall back upon but the consolidation of local authority before mentioned.

The next step in this case should be the inauguration of better arrangements for medical relief, vaccination, and other disease-prevention, under the several unified authorities; empowering them to constitute *dispensary* districts, conformably with corrected *sub-registration* districts.

The machinery for a registration of disease, attended at the public cost or in quasi-public institutions, would on this system be complete; and the returns of disease and mortality, with their causes, would be regularly and promptly forwarded for revision to the superintending Health Officer, who would report them without delay to the central authority for general use, compilation, and action. The periodical distribution of these statistics would stimulate and instruct local administration. The two orders of Medical Officers (sickness officers and health officers)



would act under uniform regulation, to be superintended by a full Medical Inspecting Staff of the central authority.

I have now sketched some leading principles of legislation and administration which might be adopted by the new Government. I have also suggested the order in which the successive steps of reform might be taken. Time is necessary for their completion. That a normal organisation could not be realised within one session of Parliament is tolerably clear; but a Boundaries Commission might begin to act at once; and a revised scheme of districts might be submitted during a following session, to be succeeded by the general establishment of dispensaries and a registration of disease, under reformed sanitary authorities, administering well-considered amendments of sanitary law, in a simplified and consolidated code. Proper areas being settled as territorial units of administration, they would have to be variously combined according to the objects to be attained. The power of combination should rest in county authorities, acting singly or jointly, subject to the approval of the central authority.<sup>1</sup> By this means, the questions of river pollution, of conservancy boards for river basins, of the great smoke nuisance in manufacturing districts, of improved dwellings for the labouring classes in connection with over-density of population, of water storage and water supply for wide and populous districts, of roads and bridges for more convenient and economical locomotion; of public institutions, as prisons and corrective establishments, workhouses, asylums, and hospitals, perhaps even elementary schools,—these difficult questions, I say, would each and all find *one* local authority in every district or

<sup>1</sup> Nothing could be more impracticable or absurd than a recent suggestion—that, instead of a compulsory power of combination, vested in a superior authority, the better plan would be to leave such compulsion to “Courts of Law”!!

The necessity for a statutory combination is shown by a quasi-official article in the *Times* of 4th April last, evidently written by one not practically acquainted with provincial districts, or he would not have failed to notice that in hundreds of instances, sanitary authorities proposed by the Inspector to be included in tolerably well-contrived districts for the appointment of an independent Officer of Health, have exercised their legal right under the Act of 1872, and have refused to join in the appointment, thus creating so many *imperia in imperio*, and rendering normal concerted administration impossible. Metropolitan officials certainly do not comprehend the geographical anomalies caused by the Act in the provinces. It is too bad to charge Mr. Stansfeld's inspectors with the discredit of these errors, clearly due to the Act itself.

combination of districts, to whom application for aid, advice, and control might be successfully made, and who would act by means of properly qualified officers—legal, engineering, and medical. Then shall Local Government and National Health become potent realities.

### THE MEDICAL OFFICERS OF HEALTH FOR COMBINED SANITARY DISTRICTS.

THE Report that has just been published of a Conference of Medical Officers of Health to Combined Sanitary Districts, which was held in London in January last, furnishes much instructive matter relative to the confusion created in sanitary organisation by Mr. Stansfeld's administration of the Public Health Act 1872. The Conference was called together by the Medical Officers themselves, in order that they might primarily take counsel as to the difficult position in which they had been placed by the Local Government Board having first advised their appointment and then taken no measures to enable them to do the duties imposed upon them; and next, that they might consider various questions relating to the execution of those duties. Among these questions the promotion of uniformity of sanitary procedure among combined Sanitary Authorities had a foremost place; and the Report rightly indicates, as one of the greatest evils arising out of the "present chaotic state of sanitary organisation," the existing want of uniformity in this respect. "That a certain amount of latitude should be permissible in carrying out in detail well-established principles, or in fulfilling clearly-defined conditions, may be granted," says the Report; "but it is clearly inexpedient, as tending to stereotype a state of universal confusion, that different Sanitary Authorities, sometimes in absolutely contiguous districts, should be permitted to adopt modes of procedure which are not only inconsistent with one another, but with well-recognised principles of science or law, with all experience of public life, and with anything like a national system of sanitary organisation."

The conclusions of the Conference are presented in a series of resolutions, numbered seriatim, but arranged in different categories according to the subjects to which they relate. The first category includes "Matters connected with the general organisation of combined Sanitary Districts;" and the first resolution refers to the necessity of constituting, "in all cases where several Sanitary Authorities have combined to appoint a single Medical Officer of Health, a Central Committee, composed of a certain number of delegates from each Authority, which should meet at least once in each year for the purpose of receiving the Annual Report of the Medical Officer of Health, and at such other times as might be found desirable, for the purpose of conferring on matters of general interest in regard to the sanitary state of the district." In other words, the combination of Sanitary Authorities has been made solely with reference to the payment of the Medical Officer of Health, and with no reference to the official performance of his duties. The Medical Officer is the officer of each several Sanitary Authority which has combined with others for this purpose, and not of a combined authority. No combination exists for the purpose of giving efficiency to the duties which devolve upon the Medical Officer of Health; and in this, the most essential aspect of the question, the combination is a mere sham. The Local Government Board has rested content for the present with dealing with this question of combination for the election of Medical Officers of Health simply as a money question—as a question of distributing the cost over the largest area—ignoring even the requirements of its own regulations: and we have here the strange spectacle of the Medical Officers appointed to these combinations themselves combining to advise the Local Government Board on its own peculiar duties.

"It may perhaps be a subject for consideration," says the Report, on the first resolution, "to what extent it would be desirable and practicable to entrust power to such a committee; but there can be little difference of opinion that even if each of its functions were limited to consultation and recommendation, it would be a very useful body, both in facilitating the work of the Medical Officer of Health and in promoting uniformity of action in the combined districts. When it is considered how much there is in sanitary work which is entirely new, how much

there is that is debateable, how much all who are occupied in it need education, and how largely that education will be promoted by opportunities for mutual discussion and by the concentration of intelligence and the conflict of opinion which such discussion would evoke, it will be seen how important the establishment of such periodical meetings is, and how desirable it is that the Local Government Board should promote this object by giving to the Committee a recognised constitution and legal status. It is by such a step that the creation of a healthy public opinion in sanitary matters would be most speedily effected, and the inertia of apathetic Sanitary Authorities most readily overcome."

The second resolution of the Conference is to the effect that "in all cases but those of Sanitary Authorities with a small number of members, each Authority should appoint a Sanitary Committee, to whom full power of action should be delegated; and that in all cases either the Sanitary Authority itself, or this Committee, should meet at least once in each month at a fixed hour for the special purpose of transacting sanitary business."

The second category of resolutions passed by the Conference related to "Matters connected with the work of the Medical Officer of Health." The first resolution in this category was, "That it is requisite that the Medical Officer of Health should be supplied with the returns of births and deaths in his district, and, so far as practicable, with those of sickness, at stated intervals; and that this should be done on a regular system and through the agency of the Local Government Board." Since this resolution was passed, the Local Government Board has taken measures to facilitate returns of the kind here referred to being obtained by Sanitary Authorities. It cannot need to be shown that the functions of a Medical Officer of Health are pretty much reduced to a farce if he is not enabled to obtain a knowledge of, at least, the mortality and the causes of death in his district. Such knowledge is implied in the very nature of his office, and without its possession the more important duties imposed upon him by the regulations of the Local Government Board could not be performed. If anyone is inclined to believe that at any time our strictures upon Mr. Stansfeld's sanitary administration have exceeded due bounds, we beg them to consider the following circular letters recently issued to Sanitary



Authorities by the Local Government Board. These letters show in the most conclusive manner how common sense has been set at nought, and existing law blundered over, to the gravest injury of local sanitary administration, and in a manner which proves how much the worst characteristics of the Circumlocution Office have been active in rendering futile the Public Health Act 1872.

*(Returns to Medical Officers of Health.)*

“LOCAL GOVERNMENT BOARD, WHITEHALL, S. W.,  
“23rd March, 1874.

“SIR,

“I am directed by the Local Government Board to call the attention of the Rural Sanitary Authority to subsections 14 and 15 of Section IV. of their General Order of the 11th November, 1872, which require that every Medical Officer of Health, whose appointment has been approved by the Board, shall prepare an annual report, comprising, amongst other things, tabular statements of the sickness and mortality in his district, and shall also transmit to the Board, on forms to be provided by them, a quarterly return of the sickness and death within such district.

“In order to enable Medical Officers of Health to discharge their duty efficiently, the Board consider it essential that they should be supplied, by the Registrars of Births and Deaths, with returns of the deaths registered within their respective districts; and the Board trust that the Rural Sanitary Authority will make the necessary arrangements for that purpose. These returns should be made weekly as regards all deaths registered as having occurred within the registrar's district during the preceding week; but an immediate notice should be given of all deaths from infectious disease in fresh localities, and of all groups of deaths from such disease, or from diarrhoea, in any localities.

“As there is no statutory provision that the registrars shall supply such returns gratuitously, and it appears just that these officers should be paid a reasonable compensation for the additional labour thus entailed upon them, the Board have consulted the Law Officers of the Crown as to the legality of the payment of such compensation by the Sanitary Authorities, and the Law Officers have advised that the Sanitary Authorities may lawfully pay for the returns referred to.

“With regard to the amount of such compensation, the Board suggest that, in accordance with a proposal made in the Registration of Births and Deaths Bill of last session, and which passed the House of Lords, it should be fixed at the rate of twopence for each death entered in the return.

“The Board direct me further to point out, that when the Medical Officers of Health are not also Poor-law Medical Officers, they will require information from the books of the latter with respect to all new cases of sickness within their respective districts, both in order that they may be cognisant of the sanitary condition of the pauper class, and may be enabled to prepare their quarterly and annual reports in conformity with the regulations of the Board; and the Board, therefore, consider it desirable that the Guardians should instruct their clerk to copy from the District Medical Officer's Relief Lists the new cases which are reported at each meeting of the Guardians, and forward the same promptly and



regularly to the Medical Officer or Officers of Health within the union. It is very important that this information should reach the Medical Officer of Health without delay, and the Board trust that arrangements will be made for the regular transmission of the copies referred to as early as practicable after each meeting.

"I am directed to add that the Board recommend that the Guardians should request the Poor-law Medical Officers to give to the Medical Officer of Health, or Inspector of Nuisances, acting within their respective districts, the earliest possible information of cases of dangerous infectious disease under their charge; as it is evident that unless such information is given as soon as the cases occur, the action of the Sanitary Authority, in regard to the prevention of contagion, must often fail in its effect.

"I am, Sir,

"Your obedient servant,

"JOHN LAMBERT, *Secretary*.

"To the Clerk to Guardians of the Poor  
and the Rural Sanitary Authority."

Now this letter deserves study, first as an illustration of the mode in which the Local Government Board does its sanitary business; next, as an illustration of the fashion in which it deals with sanitary law; and finally as an illustration of its knowledge of sanitary matters. As to the business aspect of the letter, let it be noted that although duties were imposed upon certain Medical Officers of Health on the 11th November, 1872, by the Board itself, the Board took no steps to render these duties capable of being performed until the 23rd March, 1874. Indeed, as is known from other sources, the Board actually, in the course of last year, called for the tabular statements of sickness and mortality from Medical Officers of Health, knowing all the while that such returns, owing to its own action, could not be prepared. Seventeen months after the Board imposed certain duties, it issues the instructions which render the performance of such duties practicable, and which should have preceded or accompanied the imposition of the duties! As to sanitary law, let it be noted that since the passing of the Public Health Act 1872 some of the Sanitary Authorities created by it have, of their own motion and with the assent of their law clerks, procured for their Medical Officers of Health returns of deaths and causes of death from the registrars and paid for them, but the auditors have invariably disallowed the charge. Such disallowance has caused infinite dissatisfaction, because, judging from the practice of some of the

older Sanitary Authorities, it was held to be erroneous. The mischievous effect of the disallowance need not be dwelt upon ; but now it is discovered that the law of the Board was wrong, and the law of the snubbed Sanitary Authorities right. The Board blunders mischievously for months before it does what it ought to have done at first, when a doubt had arisen, namely, seek authoritative advice on the subject. As to knowledge of sanitary matters, let it be noted that the Board has at length come to the knowledge that the prevention of contagion, as an important part of the Medical Officer of Health's duties, is really a work which should be facilitated. In fact, from beginning to end of this letter, we can perceive that the Board has been determined to deal with sanitary work as an entirely new kind of labour, and with sanitary officers as entirely new inventions, concerning both of which previous experience could teach nothing.

How far the instructions issued will meet the difficulties they are intended to overcome is doubtful. They probably include all that the Local Government Board is at present empowered to do ; and if this be the case, it is very needful when sanitary questions again come before the Legislature that the returns requisite for the efficient performance of a Medical Officer of Health's duties should be provided for. That legislation in this direction will be necessary must be obvious when it is considered that, except as to returns of deaths, the recent instructions do not apply to Urban Sanitary Authorities.

The foregoing circular is so far satisfactory as affording an earnest of better sanitary work from the new President of the Local Government Board.

Four other resolutions were passed by the Conference in the category under consideration. These resolutions were to the following effect :—With the view of bringing a district under sanitary supervision, it was held to be desirable that a detailed house-to-house survey should be made of the whole of it. It was held also desirable that a uniform set of forms for sanitary survey and other purposes should be issued for future use by the Local Government Board. It was further considered expedient that the Medical Officer of Health should be authorised generally to represent the Sanitary Authority under Section 48 of the

Sanitary Act 1866, in order to meet special emergencies; the Conference holding it to be objectionable that he should take the conduct of ordinary proceedings for the suppression of nuisances. Finally, the Conference was of opinion—water-analysis being frequently essential for sanitary purposes, and the Local Government Board having expressed an opinion that Medical Officers of Health cannot be called upon to perform analyses without special remuneration—that the Board should determine what the remuneration should be.

The third category of resolutions referred to “Matters relating to the amendment or enlargement of Sanitary Law.” The resolutions in this category will not command entire assent. The first proceeds to recite that there is urgent need for further legislation for the protection of the public health (a position which will ever be disputed); and amongst the matters for which such legislation, in the opinion of the Conference, should provide, is “the enlargement of the coercive powers generally of the Local Government Board over Sanitary Authorities, and especially in regard to the following matters, viz.: the compulsory combination of neighbouring Sanitary Authorities for objects which, in the opinion of the Board, can only be effectually carried out by such combination; and the compulsion of every Sanitary Authority to provide, either by itself or in combination with some other Authority or Authorities, a proper place for the reception of cases of infectious disease.” While admitting that greater compulsory powers might be advisable with respect to the several objects here enumerated, it may be asked, after the late experience of the administrative capacity of the Local Government Board in sanitary matters, whether the Legislature would be justified in giving increased power to this Board?

Other matters which the Conference held to require amended or enlarged legislation are thus stated:—“The enlargement of the powers given for arresting the spread of infectious diseases, especially by the enforcement of a report to the Sanitary Authority of every case of such disease immediately on its occurrence; by the compulsory isolation of all cases in which, in the opinion of the Medical Officer of Health or any medical practitioner acting on his behalf, adequate provision cannot be made by the friends of the patient for this purpose; and by the

infliction of penalties on those who, by the neglect of such practicable precautions for arresting the disease as are recommended to them by the Medical Officer of Health or any medical practitioner acting on his behalf, are the means of either directly or indirectly spreading it."

Assuming (a somewhat wild assumption) that the Legislature would give to the Medical Officer of Health the powers here asked for, would it be possible to carry them out in practice? We believe it would not be so to any useful extent, and that the very attempt would show their futility. Precautions of the kind here contemplated are practicable only on paper.

Other matters for which the Conference held that increased legislative powers were wanted were: the protection from pollution of rivers and other natural sources of water which are used or available for use for drinking purposes; the enlargement of the powers of Sanitary Authorities in regard to the supply of water to limited portions of their districts, and the compulsion of every householder to provide for his tenants a sufficient supply of wholesome water; the enlargement of the powers of Sanitary Authorities to deal with insufficiency of proper house-accommodation in their districts, and with uninhabitable houses; and, with a view of providing a remedy for these evils, the inclusion of the erection and repair of dwelling-houses below a certain value in the category of improvements which Sanitary Authorities may carry out, after full investigation and under proper restrictions, as is now done in the case of sewers, waterworks, and other similar sanitary requirements;—and the enforcement on every Sanitary Authority of the duty of scavenging all privies and other similar receptacles which are constructed and kept to its satisfaction, the occupiers of which have no convenience for so doing."

Would it be possible to devise "proper restrictions" to regulate the action of a Sanitary Authority in "the erection and repair of dwelling-houses," and which would admit of such a power being workable, without involving the Sanitary Authority in responsibilities it could not reasonably fulfil?

Another resolution of the Conference relating to the amendment of the Sanitary Laws was to the effect "That, with a view of establishing uniformity of action in sanitary matters and of

providing for the proper carrying out of statutory enactments, the Local Government Board should obtain parliamentary powers to enable it to draw up, for the use of Sanitary Authorities generally, a code of bye-laws, and that such code should contain provisions, with penalties for their enforcement, in respect of the following amongst other matters:—(a) The prevention of overcrowding; (b) the proper keeping of common lodging-houses; (c) the keeping of pigs and other animals, and the proper construction of the places in which they are slaughtered or kept; (d) the proper construction of new buildings intended for human occupation, the cleansing of privies and ash-pits, and the removal of accumulations of refuse of all kinds. That the Local Government Board should forward to every Sanitary Authority which is not previously provided with such bye-laws a copy of such code, with notice of its intention to enforce the same, subject to any additions, omissions, or amendments which the Authority may suggest, and which it shall be in the discretion of the Board to sanction.”

We congratulate the Medical Officers of Health for Combined Districts on the success of their first Conference, and we trust that it is but the prelude to Conferences of the Medical Officers of Health for all kinds of districts.



# THE PRACTITIONER.

JUNE, 1874.

## Original Communications.

### TWO CASES OF LEAD-POISONING, WITH VERY LARGE QUANTITIES OF ALBUMEN IN THE URINE.

BY E. J. SHEARMAN, M.D., M.R.C.P., F.R.S.E., ETC.,

*Physician to the Rotherham Hospital.*

*(Continued from p. 268.)*

THE gentleman alluded to in the April number of the *Practitioner*, page 266, continued to improve so much in his general health that he was determined, in spite of my remonstrance, to walk a long way every day, until he felt cold and exhausted and could barely support himself in the erect position, and complained of dizziness and disturbed vision; but his appetite kept up, and he was enabled to attend to his professional duties, which he had not done before for many months. From 23rd January to 5th March the excretion of albumen in the urine rapidly increased, and there was not an atom of urea in it, its sp. gr. being only 1002. On the 7th March, after having been exceedingly excited, he was suddenly seized with apoplexy, and died on the 9th. I much regret being refused a post-mortem examination of the kidneys.

The lady has lost all her spasmodic attacks, and is very much improved in general health; but her face, feet, and legs have become œdematous, and she is often suddenly seized with slight

attacks of vertigo. Since the 16th March the excretion of albumen in her urine has been gradually increasing, the sp. gr. being always under 1010, and often as low as 1002, with a very minute quantity of urea. There is no lead in the urine, and no blue line on her gums. I have exposed her to hot-air baths every other day, during which times I have collected from her perspiration as much urea as to be enabled to detect its crystalline flakes under the microscope most distinctly, and also proved it to be urea by converting it into nitrate and oxalate of urea. I have not yet detected any blood-discs in her urine. The head symptoms have been relieved by the hot-air baths; but the dropsical symptoms persist. She is going to leave her present residence shortly, and reside in a purer atmosphere.

Before these two patients began to drink lead water they were in perfect health. The condition of the kidneys in both seems to have been induced and kept up in consequence of the blood being contaminated by the poison of lead. I am anxious to ascertain if such cases have been noticed by any other practitioners, and, on that account, will continue the history of the present case in a future number of the *Practitioner*.

## ON THE ACTION OF PURGATIVE MEDICINES.

BY T. LAUDER BRUNTON, M.D., SC.D.

*Casualty Physician and Lecturer on Materia Medica at St. Bartholomew's Hospital.*

*(Continued from p. 350.)*

HAVING now come to a conclusion regarding the manner in which purgatives act, let us consider some of their effects upon the body. It is evident that the increased peristaltic action of the bowels will hurry along the food and cause its expulsion before the nutritive matters it contains have been fully absorbed.

If a purgative be taken immediately before or shortly after a meal, the result will be much the same as if less food had been taken or the meal entirely omitted. Many persons who are accustomed systematically to eat more than they require will regularly take a "dinner pill" or a course of Seidlitz or Pullna waters, although they cannot be persuaded to deprive themselves of a single opportunity of enjoying the pleasures of the table or to put the least restraint upon their appetites.

Increased peristaltic action will also remove fæcal matters as well as food from the intestine, and it will be greatly assisted in this by the increased secretion from the intestinal wall which purgatives induce.

I have already mentioned that mechanical irritation, such as tickling with a feather or rubbing with a glass rod, will cause secretion from the *cul de sac* of intestine in Thiry's fistula, and hardened fæces seem to have a similar action. Thus diarrhœa is not unfrequently caused by the presence of scybalous masses or other irritating matters in the intestine, and nothing cures this

like a dose of castor-oil. At first sight it seems odd that the scybala are not washed away by the fluid which they cause to be secreted, but this secretion will probably be poured out only at or below the point where they lie, and thus it will have little effect on them, though it may wash out the lower part of the bowel thoroughly enough. A dose of castor-oil, on the contrary, will induce secretion in the bowel above the scybala, and the fluid in its downward rush will carry the fæcal masses along with it.

Irritating substances in the intestine, besides acting locally upon the bowel in the manner just indicated, may exercise an influence upon distant organs through the medium of the nervous system. Sir Charles Bell<sup>1</sup> observed a case in which ulceration of the ileum was found in a man who had suffered severely from tic, but there was nothing wrong whatever with the fifth nerve, in which the pain was felt. He therefore felt convinced that although the pain was felt in the cheek, its true source was irritation in the ileum. Acting on this belief, he administered croton oil ( $\frac{1}{12}$  of a drop in combination) in tic douloureux for the purpose of removing any morbid condition of the bowel, and obtained the happiest results from its employment; and Newbigging<sup>2</sup> has found it equally efficacious in sciatica.

It is difficult to say whether the pain felt in the cheek is simply due to the irritation of the intestinal nerves being reflected, as it is termed, along the fifth nerve, or whether the irritation induces such a change through the vaso-motor nerves in the blood-vessels of the cheek as actually to set up a new irritation in the course of the fifth nerve itself. At any rate, the vessels of the face and head are very easily affected by any irritation of the stomach or intestines, as is easily seen from the extraordinary pallor which at once overspreads the face when a state of sickness and nausea has been induced. The effect of constipation in causing a feeling of fulness in the head is well known, and Ludwig and Dogiel<sup>3</sup> found that when the intestines of an animal were moved by the finger the rapidity with which the blood flowed through its carotid arteries was greatly increased.

<sup>1</sup> Bell, "Practical Essays," p. 85.

<sup>2</sup> Newbigging, *Edin. Med. and Surg. Journ.*, Jan. 1, 1841.

<sup>3</sup> Ludwig's *Arbeiten aus der physiologischen Anstalt zu Leipzig*, 1867, p. 253.

The frontal headache which so frequently accompanies gastric or intestinal derangement may possibly be due to some of the intestinal contents which ought to be evacuated being absorbed and acting as poisons on the vessels of the head themselves. I am inclined to think, however, that although this may have much to do with it, yet the headache very often depends to a great extent on some alteration in the cerebral circulation caused reflexly by the condition of the abdominal organs; for I have myself had a headache, though not a frontal one, which alternated with nausea. The nausea would last for a few minutes, during which the headache would entirely disappear; then the nausea would leave me, and the headache instantly took its place. After evacuation of the stomach, both the headache and nausea disappeared, showing that in this instance at least they were due to irritation in the stomach. But in many instances, no doubt, not only headache but much more serious symptoms may be due to the decomposition of food in the intestinal canal and the absorption of its products. Thus Senator<sup>1</sup> relates a case where a simple gastric catarrh without fever was brought on by eating something which disagreed with the patient. This was followed on the second day by great belching of gas, smelling like sulphuretted hydrogen or rotten eggs. The urine also contained sulphuretted hydrogen. As soon as this occurred the patient collapsed suddenly, and became pale and giddy, with a small, frequent, and compressible pulse. The patient remained conscious, and in a minute and a half or two minutes the collapse passed away. A similar attack came on again during the same day, but after the bowels which had been constipated were opened, the patient rapidly recovered. Senator considers that the collapse was due to poisoning by the sulphuretted hydrogen absorbed from the intestine, and it certainly seems probable that this was one cause of the attack, even if it were not the only one.

Other poisons besides sulphuretted hydrogen may be formed in the alimentary canal and absorbed into the blood, where they exert their deleterious action. Among these may be mentioned butyric acid, which has frequently been found in the stomach in

<sup>1</sup> Senator, "*Berliner Klinische Wochenschrift*," 1868, No. 24, p. 254.



considerable quantities.<sup>1</sup> According to O. Weber<sup>2</sup> it is very poisonous, exerting its action chiefly on the nerve-centres. The nervous symptoms which frequently accompany gastric derangement or disease of the intestines may therefore be frequently occasioned by poisons formed in the alimentary canal in consequence of imperfect digestion.

The administration of a brisk purgative or small doses of Epsom salts thrice a day is a most effectual remedy for frontal headache when combined with constipation; but if the bowels are regular, the morbid processes on which it depends seem to be checked and the headache removed even more effectually by nitro-hydrochloric acid or alkalies given before meals. If the headache is immediately above the eyebrows, the acid is best; but if it is a little higher up, just where the hair begins, the alkalies appear to me to be more effectual. At the same time that the headache is removed, the feelings of sleepiness and weariness which frequently lead the patients to complain that they rise up more tired than they lay down, generally disappear.

Somewhat analogous to the neuralgia of the fifth nerve in Sir Charles Bell's case, or to frontal headache, is the pain which we frequently meet with in persons having decayed teeth. The pain may be felt in the offending tooth itself, but very often it seems to give little or no uneasiness. The patients complain of neuralgic pains above the ear or along the jaw, and will often deny that they have any decayed teeth at all. It would almost seem that neither the irritation in the tooth nor irritation in the intestine alone is sufficient to produce pain, though they do so when acting conjointly; for extraction of the tooth, or stoppage of the cavity with cotton-wool steeped in melted carbolic acid, will often remove the pain although no medicine is given internally, while on the other hand a brisk purgative may also afford relief though the tooth be left untouched. It is best, however, to combine both methods of treatment, and if the tooth is not extracted or stopped, the pain is very apt to return; and it seems to me probable, though I am by no means certain of it, that this recurrence is connected with the renewal of gastric

<sup>1</sup> Kühne, "*Physiologische Chemie*," p. 58.

<sup>2</sup> O. Weber, "*Deutsche Klinik*," 1864, p. 488.

or intestinal irritation. According to Heincken,<sup>1</sup> otalgia may also depend on the presence of irritating matters in the intestine; and Sir Charles Bell observes that accumulations in the colon will give rise to pains in the loins, spermatic cord, or groin. Pain at the lower angle of the scapula is referred by him to disorder and distension of the duodenum. This pain is very often accompanied by flatulence, and is described by patients as a "pain in the pit of the stomach, shooting through between the blade-bones," and it is not unfrequently termed by them "windy spasms." It is relieved by rhubarb and alkalies given before meals.

Having said so much regarding the fæcal contents of the intestine and their local and remote actions, we must now consider a matter of no less importance, viz., the effect of purgatives upon the secretions which are poured into the intestinal tube by the various glands connected with it. The saliva which flows into the mouth from the submaxillary and parotid glands is swallowed and aids the digestion of starchy food in the stomach, and probably the intestine. A part of its active principle, ptyalin, is reabsorbed, and some of it is excreted in the urine;<sup>2</sup> but as we shall here afterwards see, it is probable that another part is excreted again by the salivary glands and thus does its work twice over. This is at present only a probability as regards ptyalin, but it is a certainty in the case of several substances which are excreted by the salivary glands, such as iodide of potassium, for example, which can be detected with great ease. When this substance is swallowed, it is absorbed from the stomach, passes in the blood to the salivary glands, and is excreted by them much more readily than by the kidneys. It again passes down with the saliva to the stomach, is reabsorbed, and again excreted. Thus it may go round and round for a long time without getting entirely out of the body. (See gastro-salivary circulation, Fig. 2.) If we wish to remove it quickly and completely, we must give a purgative so as to prevent its reabsorption from the intestinal canal by causing its speedy expulsion. The same is the case with other iodides, such as those of lead or iron. Iodine has been shown by Bernard to possess the power of making iron

<sup>1</sup> Heincken, "*De Morbis Nervorum ex Abdomine*," quoted by Sir Charles Bell, *op. cit.*, p. 93.

<sup>2</sup> Cohnheim, "*Virchow's Archiv*," xxviii. p. 250.

pass readily through the salivary glands, the iodide of iron being found in the saliva soon after it has been injected into the blood, while other salts of iron, such as lactate, never make their appearance in it at all.<sup>1</sup> Several years ago iodide of potassium was proposed by MM. Guillot and Melsens as a remedy in cases



FIG. 2.—Diagram showing the manner in which substances are excreted by one organ and reabsorbed by another, so that they circulate a long while in the organism before being expelled.<sup>2</sup>

of lead-poisoning. The lead, they consider, is present in the body in the form of an insoluble compound<sup>3</sup> which it makes with

<sup>1</sup> "Bernard, *Physiologie Expérimentale*," tom. ii. p. 99.

<sup>2</sup> The absorption of substances excreted by the salivary gland is indicated in the figure as taking place in the stomach, and their circulation is called *gastro-salivary*; but it is very probable that a considerable portion of them passes through the stomach into the intestines, and that *entero-salivary* might be a better term. Similarly, the absorption of bile has been represented as taking place in the duodenum, and that of pancreatic and gastric juices in the jejunum, but this is only to avoid confusion in the drawing, and not to indicate the part of the intestine where absorption really takes place.

<sup>3</sup> Guillot and Melsens, "*Archives Générales de Médecine*," 4th sér. iv. p. 517; and Melsens, "*Annales de Chimie*," June 1849.

the tissues, but by the administration of iodide of potassium it is rendered soluble. It then finds its way into the circulation, and is excreted by the kidneys and other emunctories. But the iodide of lead is partly excreted by the salivary glands, for M. Malherbe, of Nantes, and Dr. Sieveking have found it in the saliva of persons suffering from lead-poisoning, and who were being treated by iodide of potassium. The lead salt being swallowed with the saliva, is again reabsorbed, and thus the cure is comparatively slow when patients are treated with iodide of potassium alone. I frequently see patients suffering from lead-poisoning brought on by working in white lead, and for some time I have been accustomed to treat them with five grains of iodide of potassium, three times a day, and a sufficient quantity of sulphate of magnesia or other purgative either thrice or once a day, to keep the bowels very freely open, and cause the expulsion of the lead from the alimentary canal as quickly as it is secreted into it. I have not made comparative experiments on the effect of this treatment and of that by iodide of potassium alone, or by purgatives alone, but from what I remember of cases treated by the late Professor Syme with castor-oil, I am fully satisfied with the treatment I now adopt. The same plan would probably prove equally useful in chronic poisoning by copper or mercury.

But the gastro-salivary circle, as we may term it, from stomach to salivary glands and from salivary glands to stomach again, is not the only one in which those metals move. Their circulation in the portal system, or entero-hepatic, as it is termed by Lussana,<sup>1</sup> is still more important. (See Fig. 2.) Iron is eliminated in great part by the bile: copper and manganese appear in it also, according to Albin and Moser<sup>2</sup> and it seems probable that manganese, lead, and all the heavy metals pass out of the body by this channel. From the liver they pass into the intestine, are reabsorbed from it, and again pass to the liver and recommence their course. They may be present in considerable quantities in the blood of the portal system without reaching the general circulation or getting a chance of passing out in the urine. They are therefore much more closely locked

<sup>1</sup> Lussana, "*Lo Sperimentale*," tom. xxix. 1872.

<sup>2</sup> Quevenne, Albin and Moser, quoted by Lussana, "*Lo Sperimentale*," tom. xxix. 1872, pp. 340, 343.



up in the entero-hepatic circulation than in the gastro-salivary one, for the salivary glands are supplied by the systemic circulation, and any blood which brings lead or any other substance to them must also carry it to the kidneys. The power of the entero-hepatic circulation to retain metals within the body being much greater than that of the gastro-salivary one, it is evident that the beneficial effects of purgatives in lead-poisoning are due to their removing the metal from the portal circulation still more than their action on the gastro-salivary one which has already been discussed. Other poisons, such as curare and probably serpent's venom, may also circulate in considerable quantity in the portal system without reaching the systemic circulation, and probably this is one of the causes, though by no means the only one, which renders these substances to a great extent innocuous when swallowed.<sup>1</sup>

But the circulation of iron, lead, curare, &c., in the portal system, important though it may be, is of far less interest than the circulation of the bile itself. For the sake of convenience I have merely stated that lead, mercury, &c., are excreted in the bile, and have hitherto assumed that bile circulates in a similar way in the portal system, without giving any reason for doing so.

It used to be thought by many that bile was formed in various parts of the body, and was simply excreted by the liver. This view is now given up by most physiologists, who believe that bile is formed by the liver only. But in altering their views regarding the function of this organ they went too far, and supposed that it only formed bile, which, when it had once found its way into the intestine and mixed with the intestinal contents, became decomposed and finally expelled with the fæces. A year or two ago, however, Schiff<sup>2</sup> found that this view of the hepatic functions was too limited, and that the liver removed bile from the blood or *excreted* it as well as formed or *secreted* it.<sup>3</sup> He observed that when all the bile was drawn away from the liver by means of a fistulous opening in the gall-bladder after ligature of the

<sup>1</sup> Lussana, *op. cit.*

<sup>2</sup> Schiff, "Pflüger's Archiv," 1870, p. 568.

<sup>3</sup> Although it is not correct to do so, I use the term "secreted," here as synonymous with "formed," for the sake of conveniently distinguishing between the formation of bile in the liver and its removal from the blood.



ductus choledochus, the quantity which flowed from the liver rapidly diminished after the fistula had been established, but could again be quickly increased by the simple process of putting bile into the duodenum. The bile was at once absorbed and again excreted by the liver, and it did not make much difference whether the bile just removed from the fistula in a dog was again injected into its duodenum, or whether ox bile was used instead. In the normal state of the animal the liver is always doing two things: it is *forming* new bile, and it is *excreting* old bile which it has received from the intestine by means of the portal vessels. When a biliary fistula is made and the bile is drawn away as fast as it is secreted, none gets into the intestine, and therefore no old bile reaches the liver; consequently, the quantity collected represents only the new bile formed in the liver, and is of course much less than that which would normally pass through the ductus choledochus into the intestine. If all the bile were absorbed there would be no need for the liver to go on forming it, but this is not the case, for only a part of it is reabsorbed, and the remainder is decomposed and excreted with the *fæces*.

So long as the liver does its duty properly, and excretes again all the bile which is absorbed by the portal blood from the intestine, very little bile can pass through the organ into the vena cava and thence into the general circulation. But whenever so much bile is taken up from the intestines that the liver cannot excrete it all, it will find its way out of the portal into the systemic circulation, and will exert an injurious action on the nervous system. The same effect will follow anything which diminishes the excreting power of the liver and renders it unable to excrete the normal amount. It is evident that if anything should cause the liver to form more bile than usual at any time, it will have extra work to do in the way of excreting it after its absorption, and there will be more bile circulating in the portal blood for some time afterwards, or at any rate until the extra quantity has been got rid of or compensation has been established by the liver forming less. Many experiments have shown that an abundant supply of food causes the liver to form more bile, and we all know that heavy dinners are apt to cause biliousness. Fasting, on the other hand, diminishes the quantity of bile secreted, and everyone knows that if he fasts for a day after

taking an especially heavy dinner he may be none the worse for it, but if he dines out every night he is almost sure to become bilious unless he takes measures to prevent it by using purgatives.

It has not yet been shown by direct experiment that the symptoms usually grouped under the head of "biliousness" are due to the presence of an excess of bile in the blood; but the rapidity with which they disappear after the removal of bile from the system, either by vomiting or purgation, renders it extremely probable. Frequently we find that the fit of vomiting which has expelled a quantity of bile is hardly over when the appetite returns, the brownish-white fur disappears from the tongue, the face loses its dingy hue, the languor disappears, the irritability of temper is replaced by equanimity, and stupidity and laziness give place to sprightliness and activity. But vomiting is a disagreeable process, and few submit willingly to it, although it would be well worth while if the same end could be gained by no other means. As most old practitioners have found, however, a mercurial pill and a saline purgative produce all the good effects of vomiting without its trouble and discomfort, and they have long been in the habit of ascribing the beneficial action of the mercury to its "cholagogue" properties. They felt convinced that biliousness was due to bile in the blood, and believed that its removal was due to the liver being stimulated by the mercury to excrete the bile more rapidly. But the careful experiments made by the Edinburgh Committee of the British Association<sup>1</sup> on dogs with biliary fistula showed that neither mercurials nor other purgatives increased the flow of bile from the liver, and these results seemed at first sight to contradict the views entertained by most practitioners regarding their cholagogue action. The contradiction is apparent, but not real, for in the experiments the bile was regularly removed from the body as soon as it was formed, and none of it ever reached the intestine. Consequently, any diminution in the quantity collected simply showed that the liver was forming less. Other experiments have given somewhat different results from those of the Edinburgh Committee, and Röhrig<sup>2</sup> has found that the administration of purgatives, as well as other measures which increase the circulation in the portal

<sup>1</sup> Report of the British Association, 1868, p. 214.

<sup>2</sup> Stricker's *Medicinische Jahrbücher*, 1873, p. 250.

system, augment the formation of bile. The important question in regard to the treatment of biliousness, however, is not whether the liver forms more or less new bile, but whether the bile already circulating in the blood is removed from it. The liver may be doing its best to effect this purpose, but it will not succeed if the bile it removes from the portal blood is again absorbed as quickly as it is poured into the intestine. But if the peristaltic action of the whole intestinal canal is quickened by a purgative, the bile will be hurried rapidly onwards and evacuated before there has been time for its reabsorption, and the liver being thus relieved will be able to excrete any bile still remaining in the blood. This result will not be effected by any purgative acting on the large intestine alone, for a considerable part of the bile will in all probability have been absorbed before it gets so far; but any simple purgative or mixture of purgatives which stimulates the duodenum and small intestine as well as the large one will prove most effectual. Now, the green colour which the *fæces* present after the administration of mercurials, and which is so distinctive that the name of "calomel stools" has been applied to them, has long been regarded as an evidence of bile and appealed to as a proof of the cholagogue action of these remedies. The opponents of this doctrine have declared that the colour was simply caused by the presence of black sulphide of mercury, just as a somewhat similar colour may be occasioned by the presence of a small quantity of sulphide of iron after the administration of mild ferruginous preparations. Their statement has been disproved by Buchheim, who has shown that the colour is really due to bile, and thus established the fact that calomel induces its expulsion from the intestine. It may therefore well be called a cholagogue, and it is evident from what has already been said that it must diminish the quantity circulating in the blood, whatever its effect may be on the amount formed by the liver.

Other substances besides bile are found in calomel stools, and among the most important of these are leucine and tyrosine. These bodies are produced by the action of pancreatic juice on albuminous substances, and their presence, which was discovered by Radziejewski, indicates that the contents of the duodenum and small intestine have been expelled before much absorption

has taken place. Now, the duodenum not only contains half-digested food and bile, but also the gastric and pancreatic juices and the ferments to which they owe their activity. It is generally taken for granted that after these ferments have once aided in digesting a meal they are destroyed or evacuated, and no importance, so far as I know, has ever been attached to their reabsorption. It appears from the experiments of Brücke, who found pepsin in the muscles,<sup>1</sup> that it is reabsorbed, at least in part, and is indeed excreted in the urine, as is also a diastatic ferment derived from the saliva or pancreas.<sup>2</sup> Pancreatic ferments also are probably absorbed, for Hüfner has found some possessing like them the properties of digesting fibrin as well as converting starch into sugar in the salivary glands and lungs.<sup>3</sup> If these ferments, then, are poured into the intestine and absorbed from it again in the same way as bile, it seems highly probable that they also are excreted by the same glands which formed them. (See entero-gastric and entero-pancreatic circulations, Fig. 2.) The function of the gastric follicles and pancreas would thus be a double one like that of the liver, and they would constantly excrete the ferments absorbed from the intestine and brought to them by the blood, as well as form new quantities of them to replace those which were carried off in the fæces or destroyed in the process of digestion. This view derives some probability from the observation of Schiff, that after the stomach has already digested a copious meal and become empty its power to digest albumen is almost entirely lost,<sup>4</sup> and the fact noticed by Bernard that when the pancreatic juice is drawn away by means of a fistula, what flows from the gland some time after the operation frequently does not possess the power of digesting albumen like the juice which has been collected immediately after the insertion of the cannula.<sup>5</sup> These facts have been explained in a different way by Schiff and Bernard, but it seems to me that the explanation just given supplements without excluding theirs, and clears up some points which they have not touched.

<sup>1</sup> Brücke, "Sitzungsbericht der Wiener Academie," 1861, xliii. pp. 622, 619.

<sup>2</sup> Cohnheim, "Virchow's Archiv," xxviii. p. 250.

<sup>3</sup> Hüfner, "Journal für practischen Chemie," vol. v. p. 372.

<sup>4</sup> Schiff, "Physiologie de la Digestion," tom. ii. p. 195.

<sup>5</sup> Bernard, "Physiologie Expérimentale," tom. ii. p. 229; compare also p. 223, where he states that the juice becomes watery towards the end of digestion.



There is this important difference between the glands just mentioned and the liver, viz., that the bile can circulate in the portal system between the liver and intestine without reaching the systemic circulation, but the gastric and pancreatic ferments absorbed from the intestine cannot reach the stomach and pancreas again without mixing with the general current and the blood, and being conveyed to other organs as well. Pepsin cannot act in an alkaline fluid like the blood, but pancreatic ferment can; and although I do not know that any experiments have been made with it, yet Binz and Siegen found that a ferment derived from the liver, and possessing like the pancreatic one a diastatic power, raised the temperature of an animal when injected into it.<sup>1</sup> This rise was due to its action as a ferment, and not to its mere presence in the blood as a foreign body, for it had no action whatever when it was injected after its fermentative power had been destroyed by boiling. It is therefore quite possible that the temperature of the body is normally maintained to some extent by means of the pancreatic ferments circulating in the blood, and if purgatives diminish its quantity in the way I have supposed they will tend to lower the temperature.

It must be remembered that these are only suppositions as yet, and require much further substantiation, but they help us at any rate to form some idea of the way in which purgatives prove useful when given at the commencement of a fever. They also give us some notion of the reason why persons so often take cold after the use of purgatives, and one of the dangers of their administration to old people, who produce little heat at any rate, and can only slowly form new supplies of any ferment once carried away.

It is possible that purgatives have an additional action in remittent and intermittent fevers due to malaria, and even in continued fevers due to other poisons. Lussana supposes that the malarious poison which certainly produces some of its most marked effects on the spleen and liver, circulates like other poisons in the portal circulation.<sup>2</sup> If this hypothesis be correct,

<sup>1</sup> Siegen, "Ueber die pharmacologischen Eigenschaften von Eucalyptus Globulus." Inaugural Dissertation. Bonn, 1873, pp. 32, 34.

<sup>2</sup> Lussana, *op. cit.* p. 358.



purgatives may be productive of benefit by removing part of the poison as well as by lessening the temperature.

The pancreatic and gastric ferments have a very positive and certain use in digesting food in the intestine, even should they not possess the hypothetical action in the blood to which I have just referred; and if they are usually absorbed and excreted again, a constant course of purgatives will seriously diminish their quantity. In consequence of this, the digestion of food will be carried on slowly and imperfectly, and the general health will suffer. But this will only be the case if purgatives are used which act on the whole of the bowels, for those which affect the large intestine only will interfere but slightly with the ferments, a considerable portion of which will probably have been absorbed before they get so far. We can thus perfectly understand how a constant course of blue pill<sup>1</sup> and black draught<sup>2</sup> may have most disastrous consequences, while an aloetic pill may be swallowed nightly for months together, without doing any appreciable harm.

The experiments of Moreau and Vulpian, as well as my own, show that a large quantity of fluid is drained away from the blood into the intestine by the action of purgatives, and we can thus readily understand their use in removing fluid in dropsies. The abstraction of so much fluid will tend to empty the blood-vessels, and at the same time the irritation caused by the purgative will attract a larger proportion of blood to the intestinal vessels, and thus still further lessen the blood-pressure in other parts of the body. The blood being no longer urged onward with the same force, the congestion in any inflamed part diminishes, and the painful throbbing which is felt at every pulsation when certain parts of the body are inflamed will be diminished, or may disappear, at least for a time. The diminished tension in the arteries which purgatives induce is clearly seen from the sphygmographic tracings on the opposite page, which I owe to the kindness of Mr. Mahomed.

When the kidney is the organ affected, the benefit afforded by purgatives will be twofold, for they both diminish the work it

<sup>1</sup> Prout, "Stomach and Renal Diseases," 5th ed., p. 52.

<sup>2</sup> Pancreatic ferment appears in the fæces after the use of senna. (Radziewski, Reichert and Du Bois-Reymond's Archives, 1870, p. 72.)

has to do by eliminating water by the bowels, and at the same time lessen congestion, and thus remove an impediment to the proper performance of its function. Accordingly the administration of a purgative such as elaterium is found to lessen and sometimes to remove albumen from the urine, to render the secretion copious even when no diuretic has been given, and greatly to increase the activity of diuretics, which may have been unable to produce any action so long as the bowels were left alone.<sup>1</sup>

In conclusion I give a short *résumé* of the chief points in this paper. Purgatives act by stimulating the secretion of fluid

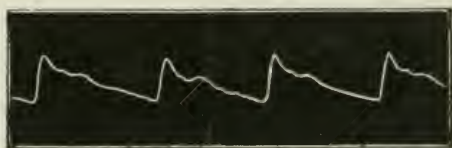


FIG. 3 is a sphygmographic tracing from the pulse of a healthy man before taking a purgative. The somewhat oblique rise, slow descent, and comparatively slight dichrotism of the pulse-wave indicate that the arterial tension is moderately high.

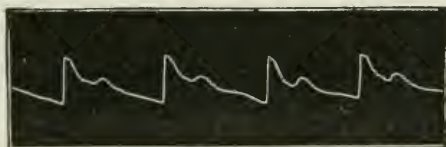


FIG. 4 is a tracing from the same person after the use of a purgative. The more abrupt rise and quicker fall, and decided dichrotism of the pulse-wave, as well as the greater frequency of the pulse, as indicated by the shortness of the waves, show that the tension in the arteries is much less than in Fig. 3.

from the intestines, as well as by increasing peristaltic action. They prove useful in many ways. They hurry the food out of the alimentary canal, and thus lessen the injurious effects of over-eating. By expelling irritating substances from the intestine they arrest diarrhoea, and remove headache and other pains, caused either by the abdominal irritation or by the absorption of poisonous matters produced by imperfect digestion and decompo-

<sup>1</sup> Geo. Johnson, *Brit. Med. Journal*, 1868, March 7, p. 215.

sition of food. They relieve biliousness by removing bile, and are most efficient aids in the treatment of chronic poisoning by lead, mercury, or other metals. It is probable that pepsin and pancreatic ferment are absorbed from the intestine and circulate in the blood, where the latter assists in the production of animal heat. They are then secreted anew by the stomach and pancreas, and do their work again. Purgatives lessen their quantity as well as that of the bile; they may thus be useful in fevers, but they injure old and feeble persons, both by diminishing their calorific power and impairing their digestion. They relieve inflammation by lowering the blood-pressure and thus diminishing congestion; and they prove beneficial in dropsies, both by abstracting water from the blood and diminishing congestion in the kidneys.

## NOTES ON POTASSIC BROMIDE.

BY W. I. MICKLE, M.D.

*Medical Superintendent, Grove Hall Asylum, Bow, London.*

THE instructive papers in recent numbers of the *Practitioner* make it matter of interest to refer once again to the influence of potassic bromide on certain complications of grave forms of insanity, or even of idiocy. A few notes are submitted with reference to the effect of this agent on—

I. Cases of inveterate epilepsy associated with idiocy or with chronic insanity.

II. On maniacal excitement, and on insomnia ; and

III. Its use in conjunction with chloral hydrate.

I. The use of potassic bromide in some cases of inveterate epilepsy associated with idiocy or with chronic insanity.

Such use of it offers one of the most severe tests to which any medicament can be subjected. In these cases, cure is hopeless; amelioration is the greatest gain to be anticipated, and little or no unmerited credit can be reaped by any special treatment from coincident improvement really due to natural decline of morbid processes or to psychical impressions. In epilepsy without mental aberration, as met with in ordinary practice, it is often difficult to decide how far a happy issue, when it occurs, is due to treatment, or how far spontaneous recovery may introduce an element of fallacy. Among the insane, also, are cases in which epilepsy, arising in adolescence or adult life, is accompanied by temporary or mild aberration, and in which recovery often supervenes on asylum residence and treatment. By excluding such

more favourable asylum cases, we retain a class in which results of treatment cannot be compared with the *cures* of epilepsy claimed for bromide of potassium by Drs. R. Reynolds,<sup>1</sup> J. W. Begbie,<sup>2</sup> E. H. Clarke,<sup>3</sup> Mr. Kesteven,<sup>4</sup> and others ; though such cures have not been observed by Dr. Anstie.<sup>5</sup> Treating only of the hopeless and inveterate among asylum cases, we cannot, of course, adduce any brilliant successes ; we cannot parallel the results obtained by many<sup>6</sup> in ordinary, or in indiscriminate alienist, practice. Voisin, however, after extensive experience, concludes that bromide of potassium can always moderate, if it cannot suspend, convulsive phenomena of an epileptic character, even when allied with idiocy and cretinism.

To avoid being tedious, I prefer (out of many) to select four typically inveterate cases, chosen for that reason to test the efficacy of treatment to the fullest, and to refer to a comparatively short period only, as during it notes were made each day with care, and all the patients were living under absolutely the same conditions. It must be added, however, that the effects noted are fairly illustrative, that the drug given for long periods in similar cases has usually maintained its reputation, though undoubtedly now and then losing part of its neurotic effects after a time.<sup>7</sup> The necessity for the *prolonged* use of potassic bromide in the treatment of epilepsy has been urged by Reynolds, Brown-Séguard, Legrand du Saulle, Voisin, Clarke, Clouston, Spender, Kesteven, and others. Hence the statement that "bromide of potassium *feeds* nerve-tissue as an antidote to convulsion, and therefore requires to be continuously given like food :"<sup>8</sup> or, that "what a splint is to a fractured bone, bromide of potassium is to an epileptic medulla . . . the bromide acts in epilepsy, as in hysteria, simply by keeping off the attacks long

<sup>1</sup> "System of Medicine," vol. ii. p. 281.

<sup>2</sup> *Practitioner*, Feb. 1874, p. 96.

<sup>3</sup> Clarke and Amory, "The Physiological and Therapeutical Action of the Bromide of Potassium and Bromide of Ammonium," 1872, p. 92.

<sup>4</sup> *Journal of Mental Science*, July 1869, p. 205.

<sup>5</sup> *Practitioner*, Jan. 1874, p. 21.

<sup>6</sup> Falret, Legrand du Saulle, Spinholtz, Voisin, Echeverria.

<sup>7</sup> Dr. C. West, Lumleian Lectures, 1871, states that the improvement often manifested at first in the epilepsy of *children*, from the bromide, is not maintained in the majority of cases.

<sup>8</sup> *British Medical Journal*, vol. ii. 1872, p. 637.



enough for the organism by constructive metamorphosis to rebuild nerve-tissue, so that the latter becomes incapable of epilepsy."<sup>1</sup> Several of the above-named authors enforce bromidal medication for one, two, or more years after the cessation of all epileptic phenomena.

The four<sup>2</sup> epileptics mentioned above resided in the same ward, under precisely the same conditions as to diet, clothing, attendance, and general management; were all free from any other bodily disorder, and for some months previously had received no medicinal treatment, though under close observation. Each was directed to take forty grains of KBr. twice a day for a month, other conditions, then and afterwards, remaining quite unchanged. The fits, mental state, physical health, morning and evening temperature and pulse, were recorded daily, and the same notes were resumed after an interval of two weeks from the omission of the bromide, in order to allow time for the subsidence of its immediate effects. It is submitted that moderate improvement, in cases such as follow, afford as strong proof of the anti-epileptic virtues of KBr. as more complete apparent success does in uncomplicated and recent cases.

CASE I.—A male idiot epileptic, age 20 years. Admitted four years previously, and described as being then violent in conduct, often assaulting those about him, and distinguishing himself by the fierce manner in which he bites all who perchance are exposed to his fury. This character he amply sustained after admission, often inflicting severe injuries on others, or beating inanimate objects in blind rage. He continued to be the subject of severe epileptic fits both by day and night, which usually occurred in bouts lasting a few days, with interspersed single fits. He was usually very irritable, quarrelsome, obtuse, sullen; and intelligence had lessened since admission. Eight days after beginning the bromide, while still having occasional fits, he became tractable, good-humoured, loquacious, and, for him, extremely sociable. For two years, at least, he had never been loquacious and good-humoured during nearly so long a period. The temperature was increased about 25°. Then a day or two ensued during which he was quarrel-

<sup>1</sup> Clarke, E. H., "The Action of Bromide of Potassium," &c., p. 94.

<sup>2</sup> For Dr. Clouston's extended investigation, see *Journ. Ment. Sci.*, Oct. 1863.

some and mulishly obstinate, though free from fits, and his temperature higher ( $98.8^{\circ}$  and  $99.1^{\circ}$ ). After this, sociability and mental ease returned. Towards the end of the month he had several fits, associated with an increase of temperature, which remained until the second day after cessation of convulsions. Thus, the thermometer in the axilla stood at  $99.5^{\circ}$ ,  $99.1^{\circ}$ ,  $99^{\circ}$ , and  $98.5^{\circ}$ ,—in each instance from one or two hours after a fit. During treatment the average morning temperature was  $.39^{\circ}$  in excess of the average evening temperature.

In this epileptic the fits were decreased in number, lessened in severity, and the mental state was beneficially affected, by the potassic bromide.

CASE II.—Male, age 55, admitted fifteen years previously, suffering from dementia, supervening on chronic mania with epilepsy, the malady being then already of several years' duration. His fits were single and not numerous, they took place either by day or night, and he was an irritable, irascible subject. While he took the bromide no fits occurred, and he was fairly tractable; but two weeks after the agent was omitted he was so excessively irritable, obstinate, and intolerant of examination, that thermometric and other observations were nearly impracticable. The few obtained, however, showed that no appreciable change was effected in the body-heat by the bromide, and no noteworthy influence produced on the pulse. Lessening of the mental irascibility caused by epilepsy was here the most marked feature of medication.

CASE III.—Male, age 25, admitted six years previously as a congenital idiot epileptic, who had then been violent and dangerous for the preceding half-year. He had severe fits, principally occurring in bouts lasting several days, and they had been frequent for several months before the bromide was given. He had one fit on the first and second days of administration; then none until the twenty-fifth day, when two occurred. The average morning temperature under KBr. was  $98.08^{\circ}$ ; subsequently, when without KBr.,  $98.07^{\circ}$ . The average evening temperature with KBr. was  $96.63^{\circ}$ ; subsequently, without it,  $96.97^{\circ}$ . The average evening temperature was, therefore,  $1.45^{\circ}$  below that of the morning with, and  $1.1^{\circ}$  without, bromidal medication.

Here the KBr. seemed to decrease the number of fits, very

decidedly. Of the four patients, this one had most fits, and their reduction was most marked. Mental irritability was lessened, and during treatment the system was in a satisfactory condition, though the evening temperature was relatively rather low.

CASE IV.—Male, age 51, admitted nine years previously, and described as being then an imbecile epileptic, dangerous during the epileptic paroxysms. He usually had one or two fits each night, whereas those of the day were infrequent; and for about three months preceding the administration of KBr. he had grown very much more stupid, dull, and demented. In him, as in Case I., the bromide caused a marked alteration in the mental complexion, for he became decidedly brighter under its use, and regained a certain amount of intelligence and sociability. He had also fewer night seizures, and no seizures by day. After he was fully under KBr. his average evening temperature was  $98.18^{\circ}$ , or  $.74^{\circ}$  above the average of the morning. This condition was probably due to the drug, for subsequently to its omission the average evening temperature was  $.17^{\circ}$  below the then existing average morning temperature.

The tendency to hebetude and stupor was obviated, apparently by treatment, in this epileptic, and the convulsive seizures lessened.

The action of the bromide in the preceding cases may be briefly noticed with reference to several points.

*Number of Fits.*—The day-fits recorded during the two months previous to taking KBr. were respectively 48 and 60 in the four cases, while during the bromide month the total amounted to eighteen only. There is not sufficient basis here for the discussion of night-fits. Dr. S. W. D. Williams<sup>1</sup> gives a strong statement as to the comparative inefficacy of KBr. for night-fits, Dr. Clouston's<sup>2</sup> investigations partly confirm this view, and Dr. Reynolds's<sup>3</sup> supports it "to a certain extent." Though much more potent against day seizures, it occasionally acts well on those of the night.<sup>4</sup> Falret, indeed, found night-fits disappear under bromide sooner than those of the day, while Echeverria<sup>5</sup> has not noticed any difference in this respect.

<sup>1</sup> "On the Efficacy of the Bromide of Potassium in Epilepsy and certain Psychical Affections," 1865, p. 15.

<sup>2</sup> *Journal of Mental Science*, Oct. 1868, p. 308.

<sup>3</sup> Op. cit. p. 281.

<sup>4</sup> *Lancet*, vol. i. 1872, p. 539.

<sup>5</sup> "On Epilepsy," p. 323.

*The Mental State.*—This was favourably modified, especially in Cases I. and IV., and it has already been noticed how exceedingly intractable and irritable Case II. became some time after the bromide was stopped, so much so that almost no observations of his temperature, &c., could then be made. The tendency of the treatment was to lessen irritability, maniacal excitement, or hebetude, and to substitute a brighter, more pleasant, and easy frame of mind.<sup>1</sup>

The general health remained good under KBr. ; appetite and digestion were unaltered in all.

*Weight of body* was diminished in none, while in two it was slightly increased,

	Weight when Bromide was commenced.	Weight when Bromide was omitted.
Case I. . . . .	140 lbs.	140 lbs.
„ II. . . . .	140 „	140 „
„ III. . . . .	118½ „	119 „
„ IV. . . . .	159 „	162½ „
	<hr/>	<hr/>
	Gain under KBr	4 „

The weight was generally found to increase up to certain larger doses at the Westmoreland and Sussex asylums.

#### *Pulse.*

Total average of the average morning pulse in the four, while taking KBr.	80·5
„ „ „ „ afterwards, without KBr.	69·6
„ „ evening „ „ while taking KBr.	81·3
„ „ „ „ afterwards, without KBr.	73·9

The reduction of pulse frequency noted by others<sup>2</sup> from therapeutic doses of potassic bromide administered to numerous epileptics would entirely negative my results derived from a few cases only, unless the explanation of the discrepancy I suggest below is tenable. According to Amory,<sup>3</sup> “the first effect produced by a moderate dose is acceleration of the pulse, which in an hour is succeeded by a retardation and diminished impulse;” and I have met with the statement of another

<sup>1</sup> Echeverria describes cases of mania caused by moderate doses of KBr. Op. cit. p. 321.

<sup>2</sup> Clouston, *Journal of Mental Science*, Oct. 1868, pp. 314, 320; Williams S. W. D., op. cit. pp. 30, 34.

<sup>3</sup> Op. cit. p. 151.

observer that KBr. accelerates the frequency of the pulse. The higher pulse-rate noticed in the above cases, while taking KBr., may possibly be due to the circumstance that their morning and evening pulse and temperature were always taken about an hour after each dose of forty grains. It is stated that toxic quantities<sup>1</sup> make the pulse slow, small, and feeble, and tend to cardiac paralysis, but we are now concerned with therapeutic doses only. In thirty-seven experiments on quiet, non-epileptic patients, I found the pulse-rate one hour after a twenty-grain dose of KBr. to vary much as compared with that observed immediately before the dose. In twenty it was lower (three of these, however, had then ceased the work they had been engaged in), in thirteen higher, and in four unchanged.

*Temperature of the Body.*—Average temperatures after the patients became fully under KBr.

With forty grains of bromide of potassium twice a day :—

	Average morning temperature.	Average evening temperature.	Evening temperature as compared with the morning temperature.
Case I. . .	98°·62	98°·23	·39° lower.
„ II. . .	96°·85	96°·75	·1° „
„ III. . .	98°·08	96°·63	1°·45 „
„ IV. . .	97°·44	98°·18	·75° higher.
<i>Total average</i>	<u>97°·74</u>	<u>97°·45</u>	<u>·29° lower.</u>

Temperatures when no potassic bromide was taken.

Average of temperature during two weeks commencing a fortnight after omission of drug :—

	Average morning temperature.	Average evening temperature.	Evening temperature as compared with that of the morning, being
Case I. . .	97°·28	97°·4	·12° higher.
„ II. . .	...	...	...
„ III. . .	98°·07	96°·97	1°·1 lower.
„ IV. . .	97°·72	97°·55	·17° „
<i>Total average</i>	<u>97°·69</u>	<u>97°·31</u>	

In Case IV., though mental state and convulsive phenomena were benefited, the normal relations of morning and evening temperature were reversed, the latter being the higher while

<sup>1</sup> Huette, Laborde, Amburger, Clarke, C. West, Echeverria, Eulenberg, and Guttman.



under KBr.; whereas when not taking it the average evening temperature was slightly below that of the morning.

On the whole, it appears that the *total* average temperature was very slightly higher with, than without, the bromide of potassium. Possibly, an explanation similar to that suggested with regard to the pulse acceleration may also apply here. Dr. Clouston<sup>1</sup> found a heightened temperature one hour after single 3j doses, but a lowering<sup>2</sup> of temperature in continuous medication, up to 150 grains a day, in three doses. At and above 150 grains daily the temperature rose again, and exceeded that usual to the same patients, and a heightened temperature was always noticed by this observer where the medicine had to be stopped on account of its ill effects.<sup>3</sup> On the other hand, Clarke<sup>4</sup> found a reduction of temperature where bromism exceeded the limits of therapeutic action and became toxic.

*Dose.*—The dose employed in the above cases was forty grains twice a day, though thirty grains three times a day is possibly a better system of exhibition. The blood, however, would be kept continuously charged with bromide by such amounts taken twice a day, if we may rely on the statement that an ordinary dose is being steadily eliminated during more than forty hours after its administration.<sup>5</sup> In the treatment of epilepsy it would appear that of various authorities, some<sup>6</sup> give 30 to 90 grains each day in divided doses; others,<sup>7</sup> from 60 to 90, or 120 grains; and others,<sup>8</sup> rather more, or about 90 to 130 or 150 grains; while Voisin prescribes 30 to 180 grains a day, adapting it to maintain that suppression of reflex nausea which he deems the criterion of therapeutic action. On the other hand, Clarke<sup>9</sup> found toxic symptoms result, as a rule, from 100 grains taken for longer than several days; and Dr. Williams<sup>10</sup> observed bad effects on several occasions, from amounts so small as ten grains twice a day. It may be

<sup>1</sup> Fothergillian Prize Essay, 1870.

<sup>2</sup> *Journal of Mental Science*, Oct. 1868, conclusion 8.      <sup>3</sup> *Ibid.* p. 314.

<sup>4</sup> *Op. cit.* pp. 37, 43, 59.

<sup>5</sup> Amory, *op. cit.* p. 138.

<sup>6</sup> Brown-Séguard, R. Reynolds.

<sup>7</sup> Clouston, W. Sander, R. McDonnell, J. K. Spender.

<sup>8</sup> Legrande du Saulle, Spinholtz.

*Op. cit.* pp. 36, 53. Echeverria makes a similar statement as to 90 grains.

<sup>10</sup> *Op. cit.* p. 10.

added that Spender<sup>1</sup> considers the frequent administration of small doses (every hour or two) of no advantage, in ordinary cases, over the administration of an equal daily quantity in larger and more distant doses.

II. The use of potassic bromide in maniacal excitement; and in insomnia.

Given *alone*, with the object of allaying excitement, restlessness, and violence, I have found two to three drachms, taken throughout the day in divided doses, to be without calmative effect in several instances of acute delirious mania, acute mania, and the wilder paroxysms of chronic mania. Added to other neurotic medicines, it has been shown by others that it greatly augments their sedative action in cases similar to those just mentioned as being unaffected by the bromide alone; but as far as I have seen, the bromide by itself, and in moderate quantities, fails to control extreme maniacal excitement. In acute mania, indeed, Dr. James Begbie observed thirty grains of bromide of potassium every second hour "reduce to quietness the restless subjects, and lay them down in sleep, of which they had for days been deprived." Only the milder cases of excitement, however, have been benefited by moderate doses in the hands of some who experimented largely and carefully. Thus we read, "The bromide of potassium alone allayed excitement in about one half the experiments in which it was used, but its effects were very much less decided in the extent to which it allayed the excitement. . . . In one half of the experiments two drachms were given, and this dose it was which had the effect on the excitement in five of the seven experiments in which any effect was observed." . . . "Bromide of potassium alone can subdue the most violent maniacal excitement, but only when given in immense and dangerous quantities."<sup>2</sup> Or, to quote the words of another observer,<sup>3</sup> "though bromide of potassium in such doses (3j—ij) is a sedative to maniacal excitement, and to a certain extent an hypnotic, yet it is not a sufficiently powerful sedative to allay intense excitement."

<sup>1</sup> *British and Foreign Med. Chir. Rev.*, Jan. 1872, p. 229.

<sup>2</sup> Dr. Clouston's Essay, conclusion 6, and experiments.

<sup>3</sup> Dr. J. A. Campbell, *Journal of Mental Science*, Jan. 1872, p. 524.

*Insomnia.*—While there exists a consensus of opinion as to the antispasmodic virtues of potassic bromide, its alleged hypnotic powers seem by no means fully accepted. It is not necessary to reproduce the testimony given to its soporific efficacy in milder forms of insanity,<sup>1</sup> nor that in its favour when used in ordinary practice, and applied with a wise electricism to the relief of insomnia dependent on causes comparatively innocuous and amenable to treatment.<sup>2</sup> Several German and French investigators failed to produce narcotic effects, even with very large doses, and recently Dr. Anstie<sup>3</sup> has called its hypnotic action in question. When used in treating insanity it has failed in my hands to abolish insomnia except that of a mild character and easily relieved by other treatment. The investigations of Dr. J. A. Campbell<sup>4</sup> show that in ordinary maniacal cases, with great excitement and insomnia, sleep followed in fewer instances, ensued later, and lasted a shorter time, from the administration of 90 grains of KBr., than from Tr. hyoscyami ʒij, or 45 grains of chloral hydrate; and more,—that in the very worst cases the bromide was utterly without effect.

### III. Bromide of potassium with chloral hydrate.

Though the bromide *alone* does not appear to be hypnotic in severe cases of insomnia with maniacal excitement and restlessness, yet the sleep-inducing power of chloral hydrate seems to be augmented by the addition of a moderate proportion of the potassium salt. Some<sup>5</sup> have used the mixture with the idea of intensifying the usual effect of the bromide by chloral, but it would seem, rather, that the bromide has the peculiar power of considerably reinforcing the characteristic effects of a number of neurotic medicines; and, among them, of chloral. This

<sup>1</sup> Dr. S. W. D. Williams, *op. cit.*, pp. 24, 34; also Fothergillian Essay, 1870, conclusion 15.

<sup>2</sup> James Begbie, *Edin. Med. Journ.*, Dec. 1866; J. Warburton Begbie, *Practitioner*, Feb. 1874; Clarke, E. H., "On Bromide of Potassium," &c., pp. 23, 27; Spender, J. K., *Brit. Med. Journ.*, vol. ii., 1873, p. 373; Behrend, H., quoted in *Journ. Ment. Sci.* from *Lancet*, 1864.

<sup>3</sup> *Op. cit.* pp. 21, 22.

<sup>4</sup> *Op. cit.* table ii. and p. 524.

<sup>5</sup> Barclay, J., *Lancet*, 1872, vol. ii. p. 405; Spender, J. K., *Brit. Med. Journ.*, 1873, vol. ii. p. 374.

view is amply supported by recent observations,<sup>1</sup> and (it is believed) is frequently acted upon in the asylums of this country.

The mixture of potass. bromide and chloral hydrate I have found of most hypnotic service in acute mania, and in chronic mania with extreme and intractable insomnia. It also often checks the nocturnal sleeplessness, noisy excitement, and destructiveness, of some general paralytics.<sup>2</sup> Out of a number of cases one or two may be mentioned.

In a case of acute delirious mania thirty grains each of chloral hydrate and potassic bromide, twice a day, proved decidedly and rapidly hypnotic. A dose of the mixture at 8 P.M. usually procured several hours' slumber; that given in the morning or towards mid-day, in from five to twenty-five minutes generally brought on sleep, which lasted, on different occasions, from twenty-five minutes to four hours. On awakening, however, the patient was always as noisy and restless as before.

Another subject of violently acute mania, when admitted, was constantly frenzied and sleepless, the senses hyperæsthetic, the head heated, the pulse rapid. KBr. ʒij and chloral hyd. grs. xl were given at once and quickly induced light sleep, which, however, was entirely dissipated by the noisy outburst of another patient. Next day, KBr. ʒj and chloral hyd. grs. xl, ordered to be taken twice a day, produced three hours' sleep after the first dose, a little rest after the second, and on the following day a slumber of one and a half hour's duration was obtained by the same means. In this, as in the case just mentioned, maniacal restlessness and noisy excitement returned unabated as soon as sleep wore off. Though the course of the disease was not directly checked in these and similar cases, the advantage of inducing a few hours' sleep and thus staving off exhaustion requires no comment. In a few chronic maniacal cases, indeed,

<sup>1</sup> Thompson, J., and Kynock, P, *Lancet*, vol. ii. 1872; Begbie, W., op. cit. p. 97; Clarke, E. H., op. cit. p. 45.

<sup>2</sup> In one such case of extreme and almost constant paralytic excitement, kept somewhat in check by moderate doses of the mixture given for six weeks, the average temperature was (to say the least) not reduced by it. Average morning and evening temperatures, 98°·2 and 98°·36 respectively. Subsequently, when the excited stage had partially subsided, the average temperatures were °6 and 1° lower.

the mixture proved calmative as well as hypnotic. It must be added that in certain instances of insomnia with melancholia, where chloral hyd. grs. xxx had failed to compel sleep, *no* success was gained by the addition of an equal proportion of bromide.



## PROGNOSIS AND TREATMENT OF CHOREA.<sup>1</sup>

BY DR. ANSTIE.

IN dealing with the fascinating subject of chorea, one is naturally tempted to plunge into the maze of those speculations which are going on concerning the pathology and etiology of the disease, but the discussion on the localisation of muscular movements in the brain, which is at present in progress, is so extremely important and goes so directly to the root of some of the main questions which are in doubt concerning the origin of chorea, that I find myself, as many other nervous pathologists probably find themselves, unable to arrive at general pathological conclusions until the questions at issue shall have been completely sifted. Upon one point, however, in the pathology of chorea I must touch, because it bears in a very direct manner upon the prognosis of the disease, viz., the connection between chorea and rheumatism; and I will dwell for a few moments on some facts observed by myself which seem to throw a new light upon this matter.

I suppose that no one will now dispute the occasional occurrence of chorea as a direct result of embolism of a cerebral vessel, since there are instances in which post-mortem evidence of the fact has been forthcoming: among others I may cite a remarkable one which was recorded by Dr. Arthur Wynne Foot in the *Irish Hospital Gazette* in the early part of last year. The possibility of an embolic origin of chorea being demonstrated, it is natural that we should be tempted to imagine embolism as actually present in all those very numerous cases

<sup>1</sup> Read before the Metropolitan Branch of the British Medical Association.

where the chorea is complicated by rheumatic valvular disease of the heart, since the conditions of embolism here lie ready to our hand, as it were. I am not going to attempt any estimate of the frequency with which chorea has an embolic origin; but I am about to suggest a different view of the connection between rheumatism and chorea, which appears to me probably more widely applicable to the explanation of the facts than the theory of embolism.

In the inquiries which I have carried on for many years respecting the pathology of neuralgia, one of the most pressing questions for solution appeared to be the kind and degree of connection which existed between neuralgia and the rheumatic diathesis. There is no need to detain you with the details of that inquiry: suffice it to say that I was compelled to the conclusion that rheumatism is comparatively rarely a direct cause of neuralgia: the truly rheumatic cases of that disease are a very limited group. At the same time, however, I began to perceive another kind of connection between rheumatism and neuralgia, which I believe will also be found to hold good between rheumatism and chorea. I observed that the hereditary character of rheumatism, which is sometimes well marked, is associated with hereditary tendencies to nervous diseases of various kinds. Upon this point the evidence (unless my experience be altogether exceptional) is very strong. My attention was first directed to the fact about six years ago, at which time I attended the third child of the same parents which had successively been under my care for acute rheumatism. The family happened to be a large one; there were many uncles, aunts, and cousins still alive, and indeed the history of the whole family stock for several generations could be fairly made out. Hereditary rheumatism was a marked feature in the history of the race, but what was far more striking was the hereditary tendencies to nervous diseases, among which both neuralgia and chorea were conspicuous. One could scarcely help pushing the inquiry as to the frequency of such a connection, and, notwithstanding the difficulty of carrying it out with completeness, I do not think that I can have been seriously misled. The facts are very remarkable. I have met during six years with nine families, in each of which the fact of two, three, or four children having

suffered from regular rheumatic fever was either witnessed or ascertained on reliable evidence by myself. In each of these instances, as might be expected, evidence was also obtained that a previous generation, or collateral branches of the same generation, had also been affected with rheumatism. All these nine families were distinguished also by the neurotic inheritance, and in nearly all of them the latter tendency had proved the more formidable of the two. The frequency of chorea was particularly marked; there was always at least one case ascertained, and where it was possible to get at the collateral branches I frequently heard of several. But this is what I particularly wish to mention:—So far from the chorea universally occurring in the victims of rheumatism, it was often strikingly the reverse; the family which first fell under my examination being a curious example of this. Here, one branch of the family, which never showed a trace of rheumatism, included two bad subjects of chorea (one of whom showed peculiar mental and moral deficiencies, to which I shall presently revert); it included also one marked subject of insanity, and a fourth member whose strange and flighty behaviour was the next thing to absolute derangement. The family of cousins among whom the three cases of acute articular rheumatism occurred, showed not a sign of choreic tendency, but included two subjects of neuralgia. And there was yet a third branch of the family in whom neither chorea nor rheumatism ever occurred, but who were neuralgic and phthisical, with tendencies to melancholia. The history of previous generations of this family had been strongly marked by tendencies to insanity and to acute rheumatism.

But I must hasten from a topic which is not formally included in the scope of my paper, though I hope to show, incidentally, that it would have been disadvantageous to omit it. I have undertaken to speak now especially of the prognosis and treatment of chorea.

The prognosis of chorea has assumed a great importance to me in consequence of what I have seen in hospital and private practice; and surely, it is a subject much too lightly treated in the ordinary systematic works on medicine. No doubt there are men who appreciate all the gravity of the subject, but they are in a minority. Most practitioners think and speak of chorea

as if it were a troublesome disease, and one in which it is not very easy to perform a rapid and illustrious cure, but otherwise of little consequence, and (but for its frequent connection with rheumatism and cardiac disease) foreboding no evil to the after-life of the patient. I wish I could even approximately entertain this view: but, not to mention the occasional fatal acute cases which occur in every hospital, I have observed a large amount of suffering and disaster to health of which chorea has either been the direct cause or at least the prophetic forerunner. I know of few things which would more incline me to think gravely of the future of a family than the fact that I found it much invaded by chorea, and there are few things which would more disincline me to think well of the health-chances of an individual than the fact that he had suffered severely from chorea, unless, indeed, there were certain accidental causes, of which we must now speak, at work in its production.

Of these accidental causes, which (from their facility of removal) enable us to look at the chorea which is their result with a favourable prognosis, the most regular in its operation is insufficient food. Where this has been the main cause of the chorea, or the chief reason why the chorea is severe, we may hope everything from the effects of copious and generous nutrition. The fact itself is of course well known, but it is not often that one gets such a forcible illustration of it as in a case that has recently been seen by me. A girl was admitted to the Westminster Hospital who was one of the most pitiable objects one could see. She was more than fifteen years old, and had already menstruated for two years; nevertheless, she was diminutive in size and exceedingly emaciated, and with a development, both of body and mind, which corresponded rather with the tenth than with the sixteenth year of life. She had extremely violent choreic movements; and as the case was one of those occurring in connection with a somewhat premature and overprofuse menstruation, I expected bromide of potassium to do much good, as it often does. But neither this nor anything else seemed to have any influence, and I was really afraid that she would either become exhausted and die, or else her mind would altogether pass into abeyance—in fact, into idiocy. The effect of steady and nutritious diet with moderate stimulation, how-

ever, told upon this child in a very unmistakable manner, and soon, instead of looking like a hunted animal, she assumed a rational expression, and the movements nearly ceased. I think it possible that at one stage she was slightly helped by zinc; a remedy, however, which I have found very unreliable in chorea. [The girl experienced a relapse after this paper was written, and her recovery was then much hastened by the treatment of ether spray to the spine, which will be spoken of presently.]

The above is an example of what is commonly enough observed: and I only mention the influence of good nourishment in order to omit nothing. We must remember, however, that though it may be easy enough to restore such patients to health for the time, a comparatively short course of their old habits of semi-starvation may reinduce the disease in a severe form, or lead (with or without chorea) to one or other of those more formidable nervous affections which are closely akin to it. And in respect to this latter possibility I wish to mention a case which has not long since been under my care, that of a young woman who had been choreic as a child of twelve years, with considerable mental disturbance. As starvation had had much to do with the precipitation, at least, of the chorea, so did generous food in the hospital very quickly produce an impression both upon the choreic state and upon the mental condition. But she went back to her poverty-stricken home for two years, and at the end of that time was prematurely forced into the slavish toil of a maid-of-all-work in a small tradesman's family. The consequence was very remarkable: she was observed to become somewhat fidgetty in her movements and sly and furtive in her gestures; and before she was sixteen she had passed (without any full re-development of the choreic affection) successively through the stages of prostitution and of religious melancholia, in which latter she settled down: the youngest of such patients that I have seen.

It is certain, however, that where we can *permanently* raise the scale of nutrition of a patient who has been brought into chorea chiefly by starvation, we may often save his nervous health, once for all.

The next, and one of the gravest questions in estimating the prognosis of chorea, is whether the affection occurs in the pre-



sexual period, or after puberty has commenced. No doubt every experienced practitioner is more or less aware of this fact, yet I think it is possible to show its magnitude and its importance more clearly than they are usually seen. I have recently had some striking examples, which, when contrasted with others which I had previously seen, illustrate our lesson with great force. The first of these was the case of a boy who was alternately under the care of myself and my colleague Dr. Farquharson at the Belgrave Hospital for Children. He was six years old, and at the time of his admission to the hospital was in a choreic state, very unusually pronounced and alarming for that time of life. There was no obvious cause for the disease. He had had no fright, nor had he suffered rheumatism. He had not had any worms. For a week before his admission he had never slept more than fifteen minutes at a time; and for a fortnight past his speech had been greatly affected. Whether it was bitten or not, the tongue swelled and became very painful: but it was obvious that this was not the real reason of his difficulty of speech, which went on increasing, after his admission, till in about a week he could not utter one word. The treatment of this case will be particularly referred to hereafter. Continuing to describe its course, I may say that with very slight fluctuations the boy went on getting steadily worse until the most alarming condition was reached. He was under our treatment from October 6th to January 16th without any real progress having been made; and during all this time it had been most rare for him to get even an hour's or a half-hour's continuous sleep. Often for twenty-four hours together he was incessantly in the most violent choreic movements, which seemed to affect every limb and almost every muscle. His incessant grimaces, dazed and terrified look, and his complete loss of speech and, as it seemed, of memory, presented a picture which, but for the one favouring element of youth, would have firmly convinced me that a speedily fatal result must ensue. From January 2nd he had been allowed some brandy (3ss every three hours), and he was less violently in movement—in fact, sometimes lay nearly still; but it seemed probable that this was only exhaustion, for at times he would be fiercely in movement for hours at a time. He did not sleep except rarely, and he

was still quite speechless and looked semi-idiotic. He was excessively thin and the pulse very weak. It was at this point that a plan of treatment was adopted of which I shall speak hereafter, and which appeared to do great good. But what I direct your attention to is the fact that the boy did recover, and that completely, and, as I believe, this recovery was due to his not having yet reached the perilous period of life which commences with puberty. The chorea had lasted nearly five months.

At the same moment that this case, about which we felt almost desperate, was in the house, another very bad case came in. This was a girl aged 9, who had had a fright about one month before admission, and had begun to have twitchings in the right side about a fortnight later. There was no rheumatic history, and no cardiac bruit; the child was a sensitive, amiable-looking creature of what one would call the sanguine-nervous temperament, with beautiful brown hair and eyes. Here, too, the movements attained to the whole body, and persisted in the most violent way, depriving the child of sleep and bringing her into a state of great depression both of body and mind, though the case was never so severe as that of the boy. Still, had she been past the age of puberty, such violent and continuous movements would have augured very badly. But in her case, too, after a variety of heroic measures had been tried quite vainly, the disease suddenly yielded in about the tenth week, and from that point she got rapidly well.

In very gloomy contrast with these cases are others which I have been unfortunate enough to see in the course of my experience. One was a girl of 17, who had menstruated from the age of 13, always profusely. She came into Westminster Hospital, not looking half so ill as the little boy whom I have mentioned; but she had not slept for several days, and was in continual general choreic movement—head, arms, legs, features, were in perpetual action. Another twenty-four hours of this made a fearful change; she got into an almost maniacal condition, and died perfectly worn out in three days from admission and in about twelve days from the beginning of the illness. And a very similar case occurred in King's College Hospital many years ago, I think under Dr. Todd: in that case a large share in the causation was due to masturbation.

But although the latter evil practice is apt most seriously to aggravate these dangerous cases, it is not at all necessary that the influence of the sexual organs should be exerted in that way; on the contrary, some of the most distinctly sexual cases that I have seen have occurred in girls who were unconscious of sexual ideas.

In the next place let me say a few words on the influence which the facts of heredity ought to exert in shaping our prognosis of chorea. And in this respect there are two things to be considered—the prognosis as to the result of the individual attack, and the prognosis as to the patient's future life. In respect to this, there are certain facts not commonly known, as I suppose. If the family from which the patient comes be on the whole strongly disposed to insanity, the chorea itself will not unfrequently be a trivial affair, but it is likely enough to be the first intimation of a coming mental degradation. I could specify to you the cases of several patients whose history has been this:—They got chorea in childhood, and they seemed to shake it off well—that is to say, they lost the peculiar movements in from one to two months. But if you watched these persons closely, you would perceive that they had never shaken off a habit of mind which first came upon them with the chorea. At first it was indicated only by a carelessness about dress, which might easily be put down to some remaining clumsiness of fingers. After a time this explanation ceased to be satisfactory, and if you still watched them closely you might find out that they contracted dirty (by which I do not mean sexual) habits. If such patients are girls, they commonly pass after puberty through a period of lazy religious excitement, which may or may not be combined with sexual excitability. Such women are sometimes saved by an early and happy marriage, which gives them a true sphere of life and a useful career. Quite as often they drift on, whether married or not, into ways of increasing shiftlessness, combined in nearly every instance with habits of personal uncleanness and defective memory. Such at least has been my experience of this class of patients.

There is, however, no rule but has its exceptions; and there is a curious fact that I wish to bring before you, viz., that the exceptions to this deleterious influence of chorea on the after-

life of persons who inherit faulty nervous, and especially faulty mental, tendencies, sometimes look like an absolute reversal of the whole principle. I mean, it occasionally happens that a boy or girl, born of a family which has numbered many nervously diseased and a few really able members, has chorea in childhood, but in place of getting intellectual harm from it, he seems to date from the period when it leaves him a most marked increase in his intellectual powers. It by no means follows that his moral nature will improve *pari passu*; indeed the spectacle of a "bullocky Orton" turning into a clever rogue like the Claimant, after a youth beset with chorea and semi-imbecility, is, I believe, less uncommon than would be supposed by most persons.

Now as to the treatment of chorea. You all know how very unsatisfactory this subject is, and small indeed is the amount of illumination that I can be expected to throw upon it in the short space of time which is at my command. I suppose that everyone who has to deal very often with chorea goes through, at least once in his life, a period of utter nihilism, in which he cannot believe that drugs have any real effect on the disease. And certainly a vast amount of positive assertion has been based solely on what was probably only coincidence, a fallacy which, when a student becomes aware of it, so disgusts him that he is very likely indeed to lose all faith in remedies from that moment.

One broad assertion which is frequently made is, that simple chorea always runs its own course in either four, six, or eight weeks, and then leaves spontaneously. No doubt it does so in very many cases, yet the longer one studies chorea the more one finds that there are many exceptions to this. Not to speak of the pretty frequent cases where chorea, interrupted for the moment by the onset of some acute disease, returns with double vigour and implants itself with double tenacity in the enfeebled organism of the convalescent patient, there is a far from inconsiderable number of simple cases of chorea which tend to drag on beyond that period of three months which, as Jaccoud justly observes, marks the limit at which chorea passes into the intractable type. I have become convinced that there are several means by which at least the



disease can be kept to the shorter and more normal term. And besides this, I do not doubt that we can sometimes intervene in the terrible acute cases, with the effect of saving life and preventing the patient from becoming imbecile or epileptic.

In commencing the subject of treatment it is necessary to remark that if embolism be considered the probable cause of any given case of chorea, medicinal treatment must surely be vain. Tonics and cod-liver oil may possibly be of some use in improving nutrition, but we must necessarily wait for the removal of the disease by natural processes. When, therefore, a person who is notoriously suffering from valvular disease of the heart suddenly gets an attack of some kind, paralytic or epileptic, and thereupon passes into a state of chorea, there can be no sense in adopting any special plan of treatment beyond that already indicated.

In the very numerous cases, however, in which there is neither rheumatism nor heart-disease present, we should be very foolish, in my opinion, to give up the attempt to interfere with the disease; and indeed the great discomfort which the patient suffers, and the alarm which his friends experience, will not allow us to fold our hands. I wish therefore to mention the few things which I have found really useful: and in the first place we will take the gravely threatening acute cases, such as those of the two children in the Belgrave Hospital who have been already referred to. The boy, aged 6, is a remarkable monument of what the organism will endure from physicians. We were determined to test the power of *succus conii* to the utmost, and, commencing with ʒvj daily, we got up to 8 ounces daily of the *succus* without producing any more effect than as if so much water had been given. I wish particularly to mention that the preparation was got from three different and first-rate chemists in succession, so that there is no reason to believe that we were employing an inert specimen; indeed, I believe our house-physician proved in himself the physiological activity of the sample of which the patient took 24 ounces in three days. The same result happened with the girl who was under treatment at the same time; however, she never got beyond three ounces of the *succus* daily.

In the excessively severe case of the boy, we tried not only



conium, but bromide of potassium, camphor, ol. morrhuæ, and zinc in large doses, all with only momentary effect. We then tried Jaccoud's plan, which I have found extraordinarily successful in several cases: viz., we sprayed the skin over the whole length of the spine with ether twice daily. I will not positively say that it was *propter hoc*, but I will say that immediately *post hoc* the symptoms greatly amended, and in the course of a fortnight the lad was perfectly free from movements. We kept him in the hospital several weeks longer, in order to re-educate his speech, which was grievously deficient. But by the time he left the house he could speak quite well and manage all his limbs; nothing amiss remained beyond a slight tendency to grimace. The girl, with whom succus conii, camphor, ol. morrhuæ, bromide of potassium, and large doses of zinc had entirely failed, began to improve immediately on taking liq. arsenicalis in 5-minim doses, afterwards reduced to 3 minims. I am convinced that in one of these cases death, and in the other a protracted and very serious illness, were avoided by the use of remedies; and I will just say here that arsenic as an internal remedy, and the ether spray applied to the spine, have given me solid results such as have been obtained by no other remedy. The ether spray stands somewhat intermediate, I suppose, between the ordinary shower-bath and the spinal ice-bags of which so much has been said. Cod-liver oil and iron, however, are very useful in anæmic and generally debilitated subjects. And there is a special class of cases connected with violent ovarian excitement, or complicated with epileptic tendencies, in which the bromide of potassium is invaluable, and is the one remedy.

In the terribly dangerous acute cases of young women, especially where there has been sexual excitement and exhaustion, I believe nothing does any good but free stimulation, regular feeding per rectum, and opium in large doses. I regret to have to express my complete distrust in chloral, and in a host of other remedies that have been proposed.

In the milder stages, and especially with a view to the future bodily and mental health, there are a variety of precautions which ought to be adopted.

Under these circumstances we often get the first considerable improvement by the use of the shower-bath. This old-fashioned

and useful remedy is a two-edged weapon, and if employed in cases where the nervous system is too severely disordered and prostrated may produce bad results ; but in the milder and more chronic type of chorea it is still worthy of being called a sheet-anchor. It gives that preliminary bracing to the nervous and moral tone, without which we may fail to get a leverage for other treatment. There is no absolute necessity for beginning with cold water, and in delicate subjects it is better at first to use it tepid ; but we can soon advance to the cold shower-bath daily or twice daily. The second item of treatment is the training of the muscular system. We can do much more good with this, in most cases, than with medicine, provided that the patient's nutrition is kept thoroughly good. This lesson has been particularly enforced by my colleague Dr. Radcliffe, who always insisted much upon the value of muscular exercises which require rhythmical movements. He used, in Westminster Hospital, to give the choreic children skipping-rope exercise ; and this will be found a very useful thing either in the absence of means for more elaborate training, or as introduction to more studied and complicated movements. I may conclude by pointing out the necessity for careful training in speech, where that faculty has been at all impaired, and also of attention to mental education in every case. The mere cessation of the choreic movements ought not to make us consider that we have done our work ; it is most important that a judicious system of education should be at once adopted to strengthen the mind, and especially the memory. And although every care should be taken to avoid harsh or unkind treatment, it is very needful that a firm and regular discipline should be established, for anything like slackness or vacillation on the part of the educator is sure to be reflected and exaggerated in the behaviour of a nervously weak child.

## ON "REST" IN THE TREATMENT OF CHEST AFFECTIONS.

BY FREDERICK T. ROBERTS, M.D., B.SC., M.R.C.P.

*Assistant Physician to University College Hospital, and to the Brompton Consumption Hospital.*

*(Continued from p. 335.)*

As bearing upon the question of the value of strapping the side in cases of acute pneumonia affecting one base, since writing the remarks upon this subject in the last number of the *Practitioner* I have had the opportunity of treating one very typical case in this way, and certainly the result has been encouraging. The patient was a youth, aged 19, strong and healthy-looking. He was brought to the out-patient room of University Hospital, on April 15th. It was stated that he had had a fall a few days previously, but his illness did not appear to be at all connected with this. Without entering into details, it will suffice to state that he was found to be suffering from acute pneumonia affecting the left lower lobe, in its early stage. Breathing was much distressed, the pain in the side was severe, and the general symptoms were highly marked, the temperature being nearly  $103^{\circ}$ . I thought the case was a very appropriate one in which to try the effects of strapping the side, but there was no bed vacant in the medical wards. As the patient lived in the neighbourhood, however, I determined to treat him at his own home, and Mr. Skerritt, physician's assistant at the hospital, kindly undertook to visit him daily. His side was then carefully strapped in the same manner as for pleurisy, and he was sent home, and ordered to remain in bed. The only medicine

given was a solution of citrate of potash. The patient expressed himself as relieved at once, and appeared decidedly more comfortable. He was seen the next day, and he then felt quite easy, and did not think there was much the matter with him. The physical signs of pneumonic consolidation were very typical over the affected base, and continued so for a week, after which resolution was rapidly and completely effected, and the patient was soon quite convalescent. There were really scarcely any obvious symptoms throughout the case, after the plasters had been applied, and the patient could hardly understand why he should be kept in bed. So far as could be ascertained, no evidence of any crisis appeared. I do not know whether the treatment had any influence upon the course of the disease, but at all events I am convinced it enabled the patient to pass through his illness as comfortably and satisfactorily as he possibly could, and so far was decidedly useful. Whenever a suitable opportunity offers, I shall certainly feel disposed to try the same treatment.

Two of the most important and most common of the diseases of the pulmonary organs remain to be considered, viz., *Emphysema* and *Phthisis*, and there are some practical points connected with the subject of rest as applicable to the treatment of these affections, the discussion of which may, I trust, prove not altogether devoid of interest or usefulness.

EMPHYSEMA.—This term has been applied to morbid conditions of different kinds, and having very different modes of origin. It is of the *chronic hypertrophous* variety that I wish to speak more particularly here, that in which a greater or less number of the air-vesicles are permanently distended, as the result of some force acting upon their interior, their walls, at the same time, frequently presenting serious organic changes. Hence the lungs are more or less enlarged, and cannot collapse properly and efficiently.

Without entering upon any discussion as to the causes of this form of emphysema, either remote or immediate, I lay down the following propositions, bearing upon the subsequent remarks concerning treatment, and can scarcely imagine that they will be called in question:—1. The large majority of cases which

come under observation in practice, at least such as are of any importance, are those in which the morbid condition results from bronchial catarrh, particularly where this is present as a chronic complaint, or occurs in frequent attacks. 2. Without necessarily assenting to the *Expiratory* theory of the production of emphysema, as applicable to all cases, it cannot be denied that acts which are attended with forcible expiratory efforts, such as cough, especially if there should be at the same time some obstacle to the escape of air from the lungs, must tend to cause distension of the vesicles, at least in certain portions of these organs. 3. In proportion to the degree in which the walls of the air-cells and minute bronchi are involved in pathological changes which impair their elasticity and diminish their resisting power, will the emphysematous condition be more likely to be developed by anything that causes an internal strain upon them.

The extent of pulmonary tissue involved in emphysema varies considerably. Very commonly in post-mortem examinations limited portions about the apices and margins of the lungs are found affected, but this has given rise to no trouble during life. On the other hand, both organs may be more or less emphysematous throughout. Now there are certain points which I believe it is highly important to insist upon here, and which should always be kept prominently in mind: one of the great objects as regards the treatment of emphysema should be to use every possible means for preventing its extension. Once this morbid condition has been decidedly developed in any portion of lung, I do not believe that the affected part can be restored to its normal state, and the more widely spread the mischief is, the more difficult does it become to do any good. Emphysema, if at all extensive, is a very distressing complaint, and it is among the most troublesome of chest affections one has to treat. Therefore, though certainly much may be effected by judicious management even in advanced cases, in the way of alleviating the condition of the patient and making existence more comfortable, yet the great aim should be to check the progress of the mischief, and this ought to be kept in view from the first. I draw particular attention to this matter, because my experience of a large number of emphysematous cases has



convinced me that many of them are the result of neglect and carelessness, and that the progress of the disease might at least have been materially checked by proper precautions. This is especially distressing in the case of children and young persons, of whom not a few have come under my notice, who suffer seriously from emphysema, and who are destined to pass through a miserable existence, simply on account of neglect. Therefore I repeat emphatically, that if any patient is found to present any tendency to emphysema, prevention should be the first principle of treatment. The earlier in the progress of the case this can be carried out the better, and it demands particular attention, should the patient be habitually exposed to certain well-known causes of the complaint, or should there be reason to believe that the walls of the air-vesicles are the seat of degenerative changes, as in old persons.

I proceed now to point out what I consider of most consequence in the treatment of emphysema, with reference to the question of rest, particularly with the view of arresting its progress.

1. Remembering its frequent association with bronchial catarrh, it is obvious that in all cases of emphysema, however limited, every known cause of this complaint should be carefully avoided, and in case an attack does come on, prompt and efficient measures ought to be taken to cure it as soon as possible. This matter deserves much more attention than it is accustomed to receive in all classes of patients, but particularly in the case of those who are subject to chronic or frequent attacks of bronchitis, or who are at all advanced in years. Every successive attack increases the mischief, and the more emphysematous the lungs become, the more easily is the catarrh excited in turn. I have no hesitation in affirming that many serious cases of emphysema owe their origin to, or are greatly aggravated by, a want of attention to what I have just been urging, and it is on this account that I give it a prominent place in these remarks.

There is one form of bronchial catarrh which demands particular care, and that is the so-called "dry catarrh," in which various dry râles are heard extensively over the chest. This is often very difficult to get rid of, and it certainly is more

liable than any other form to give rise to very considerable emphysema.

Bronchial catarrh being so often caused and kept up by the air which is habitually breathed, it is all-important in every case of this kind to see that, so far as circumstances will permit, no irritation shall arise from this source. In numerous instances the whole mischief is due to the atmosphere inhaled, particularly when this is impregnated with irritating solid particles, in connection with certain occupations. Now, if in any case it is found that the employment of the patient entails the breathing of irritating air, and that this is causing more or less persistent catarrh, which is in turn originating emphysema, the first advice that ought to be given is, that such employment should be relinquished at once, if this is practicable. I wish to draw special attention to this matter in the case of young persons. Unfortunately the circumstances of adult patients often forbid their following this advice; but I do very strongly urge, that if a boy or girl (it is generally the former) is affected injuriously by his or her occupation, in the way mentioned, it is the duty of the practitioner to insist upon the necessity of a change, for it is infinitely better to cause a little inconvenience than to allow a patient to drift into what is sure to be a very uncomfortable existence, to say the least. When the employment cannot be given up, then all that can be done is to enjoin all possible carefulness on the part of the patient against inhaling the irritating air, and to recommend the use of some protecting apparatus over the mouth and nose, or such other measures as the circumstances of the case may suggest. Unquestionably sufficient attention is by no means paid to these precautionary measures; they are not brought under the notice of patients and others at all so prominently as they ought to be, and in many occupations more stringent sanitary regulations are called for, so as to compel employers of labour to adopt every means in their power for the protection of disease from this cause.

There is one other point, having reference to the air inhaled, to which I desire to call attention, and it relates to change of climate or residence. There are not a few cases of chronic bronchial catarrh in which recovery seems to be impossible in

this country, or in some parts of it, especially during the colder seasons. If these patients improve at all, they are certainly liable to exacerbations. This is partly due to the coldness and dampness of the air breathed, and when in this condition it retains more readily various irritant particles which float about. What I wish to insist upon is, the great importance of recognising this fact, of remembering that in these cases the *one thing* needed is a change to some more suitable climate, either abroad or in the more genial parts of this country, and that the longer this is delayed, the more extensive and permanent will be the organic mischief set up in the lungs. In short, if a case of chronic bronchial catarrh, particularly if accompanied with emphysema, does not seem to improve under a fair trial of appropriate remedies, the sooner a change of climate is ordered the better. The beneficial results of such a change are not uncommonly really wonderful. If the circumstances of the patient will not allow of this, the next best thing is to recommend confinement to a comfortable, warm room during cold or damp weather. To attempt to cure patients suffering from obstinate bronchitis with emphysema, while they are constantly or frequently obliged to breathe cold and damp air, is out of the question.

The causative relation of irritating air to bronchial catarrh is one reason why I have written so much about this complaint in connection with the subject of rest as applied to the treatment of emphysema. Another reason lies in the manner in which the catarrh leads to emphysema. Most certainly the frequent and often violent fits of coughing which it excites, contribute one very important element in the causation, though not the only one; and therefore by preventing or allaying this morbid condition, one of the chief indications for giving rest to the lungs is fulfilled.

2. In all cases of established emphysema, as well as where there is any tendency to this disease, it is of great consequence to restrain as much as possible all actions attended with forcible expiratory efforts. Among those to be specially mentioned are public speaking, street-shouting, singing, playing musical wind-instruments, straining at stool, and, above all, violent coughing. As to the management of *cough* in cases of *chronic* bronchitis,

This must be decided by circumstances, but the cough is generally much more severe than is required for the discharge of the sputa, and consequently needs to be moderated. Hence in many of these cases sedative medicines are decidedly serviceable when administered under proper supervision, and inhalations of this nature are particularly useful. There are two other practical hints to which I would call attention here. Very commonly bronchitic patients can voluntarily repress cough if they choose to do so, or rather they frequently of their own accord start off a fit of coughing, having a feeling that there is something in their air-tubes which they must get rid of, and to accomplish this they make vigorous exertions. This occurs especially in the mornings, and it tends to do much harm, while it only distresses the patients, causing them to feel exhausted and breathless, without giving them the relief they seek. I have many a time obtained much benefit by impressing upon patients how useless and injurious these efforts are, and how much better it is for them to humour the secretion, as it were, only trying to get rid of it when it really comes into the main air-passages. Again, in these cases catarrh of the throat is often present, and excites much cough; and treatment directed to this part, in the way of astringent gargles, lozenges, glycerines, &c., or other remedies, may relieve this symptom very materially.

It is not, however, only in connection with bronchitis that cough may be so violent as to produce emphysema. The constant inhalation of irritating air may, quite independently of affecting the lungs, excite much cough by mere irritation of the throat and larynx, and may thus originate emphysema. Diseases of the larynx also, which are attended with severe fits of coughing, may certainly be the cause of this disease, especially if at the same time any obstruction is offered to the escape of air. Indeed, mere throat-affections which give rise to much cough (and they are very common) may thus lead to organic mischief in the lungs. In all cases of this kind, therefore, and any others in which cough is a prominent symptom, it is important not to allow it to continue unchecked, but to adopt appropriate measures for its suppression.

3. Another matter of importance in the management of cases of emphysema, if this is at all extensively developed, is to limit

all kinds of physical effort as much as possible, and this is especially needed when the disease is far advanced. Emphysematous patients should be given clearly to understand that they are not, and never can be, capable of exertion, and that the quieter they keep the better it will be for themselves. Any extra work thrown upon the lungs when in this condition must tend to injure them still further, while the effort causes the patients much distress. One of the commonest complaints among this class of individuals at the Brompton Hospital is, that they get so short of breath when they walk a little fast, or up a hill, or do anything that entails any exertion. Instead of encouraging them to expect any improvement in this respect, I try to impress upon them that they must not attempt to do anything of this kind, and that they must study to keep themselves as much at rest as possible, never doing anything which can hurry the breathing.

4. I just mention, lastly, the *inhalation of condensed air* as a mode of treating emphysema. That it does good in some instances seems clear, but how it acts is not so evident. Probably the rest it gives to the lungs may be one of the elements to which the beneficial effects of this treatment may be attributed, but there are others which perhaps are of more importance.

(*To be concluded.*)

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WE have been requested to state that the very interesting manuscript volume of Reports of a Medical Society in the country, from which we were permitted to republish Dr. Allvey's paper on the Cold Water Treatment, in the April *Practitioner*, was unearthed by the diligence of Dr. Saul, of Charlotte Street, Fitzroy Square, and by him given to Dr. Wilson Fox.—ED. *Pract.*



## Reviews.

*Clinical Aspects of Syphilitic Nervous Affections.* By THOMAS BUZZARD, M.D., F.R.C.P.; Physician to the National Hospital for the Paralysed and Epileptic. London: Churchill, 1874. 8vo. pp. 148.

THIS treatise has been written to supply a want which has been much felt, and it is the work of a competent observer with ample supplies of the requisite material at his command. It will be admitted by all that a short and yet sufficiently clear account of the syphilitic affections of the nervous system was a desideratum with English practitioners; and we think that this book successfully meets the requirements of the case. It would have been possible, of course, to make a very much larger book on the subject, but it is doubtful if this would be really useful. For advanced experts there is no need of a new work on syphilis, as affecting the nervous system, until some fresh points of high importance shall have been made out; but the very grievance of the English medical profession was that there was no handbook conveying a broad general view of what every practitioner ought to know respecting this very serious group of diseases.

Few things in the way of pathological discovery within the present century have been more important in themselves, or more calculated to encourage practitioners in their daily work, than the new knowledge respecting the syphilitic origin of many nervous diseases. A large class of particularly obscure maladies, as to which treatment was entirely haphazard, and the expectation of success was very small, have been placed in the list of readily intelligible and curable diseases. It only remains that the knowledge of the new facts should be widely diffused; and for practical purposes it will be found that Dr. Buzzard's book is sufficient to convey all the required instruction. The general style of the work is good; that is to say, the language is clear and simple, and the construction of the sentences elegant. The division of the subject is convenient; the book consists of four chapters, respectively dealing with diagnosis, pathology, and morbid anatomy, illustrative cases, and prognosis and treatment.

And when the reader turns to the clinical part, which may naturally be considered as affording a good test of the author's personal knowledge of the subject, he will perceive that the thirty-seven cases recorded have been observed in a scientific spirit, and selected with good judgment to illustrate the principles laid down in the book.

In the chapter on diagnosis, Dr. Buzzard has occasion to repeat a truth which is a truism to the few, but is disregarded, or practically denied by very many, viz., that syphilis ought always to be considered as a possible cause in cases of obscure nervous disease where its existence is not manifestly impossible, and that the absence of a history of the usual external symptoms ought never to be accepted as a disproof of the syphilitic hypothesis. We are inclined to dwell a little on this point, for we often read or hear remarks which show that medical men distrust, on insufficient grounds, the soundness of the diagnoses of syphilitic nervous affections which are made by those who have special experience in the matter. It will be impossible to convince the mass of practitioners of the frequency with which syphilitic disease is the origin of nervous maladies except by the reiterated declarations of experts; and we therefore welcome Dr. Buzzard's book as a new weight in the right scale. The present writer has a strong interest in these questions, for it is now nineteen years since he wrote, as a student, a small essay on syphilitic paralysis (now deservedly buried and forgotten) which was among the earliest English notices of the subject. Since that time syphilitic nervous disease has been constantly acquiring a greater importance in his mind, as the result of increased experience; and he would decidedly endorse the opinion that if the physician waits to act in an antisymphilitic sense until he obtains a clear and unmistakable history of past syphilis, he will lose many lives, and leave uncured a great deal of perfectly curable nervous disease. In the bizarre grouping of symptoms, which is so often seen in syphilitic nervous disease, we possess, as our author justly points out, a means of tracking back to syphilis as the original cause as surely as if we had with our own eyes observed the typical development of the latter disease in our patient's system; and although the mere fact that antisymphilitic remedies remove nervous phenomena does not in itself prove the specific origin of the latter, it affords a powerful reinforcement to diagnostic arguments drawn from the characters of the disease.

Among the subjects dealt with in this book there are none more interesting to ourselves than the remarks on ophthalmoscopic appearances; but on this point we do not feel competent to offer any important criticism. A matter with which we are more familiar is the diagnostic value of electricity, and upon

this point there are many things which we should like to say if there were time. We shall limit ourselves to one observation. Dr. Buzzard correctly remarks that, as a general principle, the muscles, in syphilitic paralysis, conform to the usual rule of peripheral paralysis as regards irritability to the faradaic current: the irritability is lost. But there certainly are some syphilitic paralyses, with wasting of a most characteristic kind, in which the faradaic irritability is retained, and in which, nevertheless, it is very difficult to believe that the lesion was actually in the nerve-centres. Such was a case read before the Clinical Society by Dr. Anstie (*Transact. Clin. Soc.*, vol. iv.), in which a number of muscles successively wasted, and were repaired, with great rapidity, the faradaic irritability being retained; this case was pronounced by Dr. Hughlings Jackson, among other competent auditors, to be in all probability one of syphilitic pachymeningitis.

As regards that part of Dr. Buzzard's book which relates to therapeutics, we can naturally speak with pleasure, since any competent account of the treatment of syphilitic nervous affections must necessarily contain most cheering statements of the success of remedies in apparently desperate circumstances. The list of remedies is not large; nevertheless it will be desirable for the practitioner to weigh well every word that is stated on this subject by an experienced observer; and this he should especially do as regards doses. We are glad to see that the author speaks decidedly as to the necessity of frequently using iodide of potassium in very large doses: he says that he has several times found a case remaining without improvement till the iodide was increased by successive steps to 30, 40, 60, or even 90 grains three times a day, and then making rapid progress; and he reminds us that Elliotson used to give enormous doses (120 grains thrice daily) with excellent effect. We observe that Dr. Buzzard reports unfavourably on the subcutaneous employment of Staub's albuminate of mercury: but for our own part we continue to hope that some practicable method may yet be found by which mercury may be used in the hypodermic method, which has tended so much to increase the value of other medicines. The few remarks on electrical treatment of paralysed muscles are valuable, and might have been expanded with advantage. We observe that Dr. Buzzard speaks well of the practice of faradising paralysed ocular muscles through the shut lids, a point as to which there have been conflicting statements.

In concluding this notice we may express the opinion that this work ought to be followed by other contributions to the pathology and treatment of nervous affections, drawn from the great storehouse of facts which lies open to Dr. Buzzard and his

colleagues of the Queen-square Hospital. They have scarcely, as yet, allowed us to reap the whole benefits which we may expect from the unique opportunities of observation which they possess. The present little work on "Syphilitic Nervous Affections" is, however, a valuable instalment; and we have confidence in recommending it as a reliable guide to the diagnosis and management of the important class of diseases of which it treats.

[Owing to the necessity of anticipating Whitsuntide, our reviews of other books are postponed.—Ed. *Pract.*]

## Extracts from British and Foreign Journals.

**The Physiological Action of Apomorphin.**—Max Quehl, in an inaugural dissertation, gives the results of a series of experiments he has performed, under the guidance of Professor Köhler, on the effects of apomorphin-muriate upon frogs and dogs, the alkaloid being applied partly in the form of powder, and partly in a solution containing 1:50 parts of water. He finds that when subcutaneously injected, which is the best method of administration on account of the small amount of irritation it excites, vomiting is caused in dogs by a dose not exceeding  $\frac{1}{2}$  to 2 milligrammes; in man, from 6 to 7 milligrammes are requisite. When given by the mouth in dogs, 3 to 4 centigrammes constitute a certain dose, but 1 centigramme is uncertain. In the case of man, 12 to 18 centigrammes are required to produce the effect with certainty. When introduced per anum, 6 centigrammes are required in the case of the dog, and in man from 18 to 36. Placed upon the tongue from 1 to 2 centigrammes are required for dogs. Infraction of apomorphia ointment into the skin of the inner side of the thigh, and the introduction of plugs with apomorphin ointment smeared over them, into the vagina, had no action. The experiments upon the effects of the introduction of large doses of apomorphin,  $1\frac{1}{2}$  to 6 grains subcutaneously, gave interesting results. No vomiting followed them, but the drug had a narcotic effect. The animals performed the movements of *manège* and running round in a circle; there were reduction of reflex excitability and paresis of the hind legs. The pupil was frequently dilated, and, as a subsequent effect, loss of appetite, which remained for two days, and general depression. Death did not occur in any case, even when large doses were administered. The effects, after moderate emetic doses, upon the pulse, temperature, and respiration, as observed by Quehl, agree precisely with those observed by Siebert. Nutrition underwent no change; so that an animal made to vomit once daily at least by means of apomorphin for at least several weeks, exhibited an increase of  $2\frac{1}{2}$  lbs. In regard to the action of apomorphin upon



the nervous and muscular systems, and upon the circulation, Quehl thus sums up his results :—1. Apomorphin affects neither the motor nor the sensory nerves. 2. After section of the vagus, its emetic effect ceases. 3. The curve of the muscles in contraction is not in any way modified in the case of the frog. 4. There is no evidence of any influence upon the vaso-motor nerves, nor of any paralysing influence upon the reflex action of the sensory nerves upon the vaso-motor centre through the spinal cord. The main conclusion to be drawn from the whole of the experiments is, that apomorphin in small doses is to be regarded as the best and least dangerous of all emetics. In the discussion at the Leipsic Society of Natural Sciences, after the reading of the paper, Mosler stated that he agreed with the author in regard to the emetic properties of apomorphin in fresh solution, but that he had found that when the solutions were five or six days old the phenomena of intoxication with the drug occurred. Köhler stated that he had found that after some weeks, though the solution had acquired an intense emerald-green colour, it yet had lost little of its activity. Riegel, who had made upwards of seven hundred experiments with apomorphin, said that he did not regard it as very decomposable, and that, in opposition to Quehl, he had noticed vomiting after section of the vagus. The vomiting, he thought, must be explained on the idea of the action of the drug on the central nervous system, since, after a series of sections of the spinal cord, he at length arrived at a plane, after which no vomiting follows. (*Medicinische-Chirurgische Rundschau*, Heft ii., 1874.)

**Pharmacological Action of certain Uniatomic Alcohols.**—Professor J. Dogiel, of Kasan, at a recent meeting of Russian *savans* held in that place, gave the following as the results of a series of investigations he had made :—

1. Æthyl-alcohol introduced into the stomach is absorbed not only by the veins but by the lymphatics. Alcohol can be demonstrated in both arterial and venous blood, and in the lymph of the thoracic duct, about one minute and a half after ingestion.

2. The excretion of alcohol from the organism is effected in the frog not only by the urine and feces, but also by the skin.

3. Æthyl-alcohol acts not by reduction of the blood, *i.e.*, not by the products of the oxidation in the blood, as by conversion into acetic acid or aldehyde, but by its own specific action.

4. The action of alcohol upon blood withdrawn by blood-letting varies considerably, both in reference to its coagulability and to the escape of hæmoglobin from the blood-discs and its crystallisation. Blood allowed to flow from a vein into a vessel containing alcohol of from 20 to 40 per cent. either does not coagulate at all or only very loosely, whilst 97 per cent. alcohol

causes even defibrinated blood to coagulate. The best means of obtaining hæmoglobin crystals was found by Dogiel to be, to add 2·3 cc. of alcohol of 97 per cent. to 10 cc. of defibrinated blood, and allow the mixture to stand for twenty-four hours at a temperature of from 14° to 17°C.

5. The crystallisation of the hæmoglobin under the influence of alcohol does not depend upon the action of oxygen. Blood charged with CO or CO<sub>2</sub> crystallises simultaneously with arterial and venous blood.

6. The blood of an animal under the influence of alcohol crystallises more slowly and gives less fibrin than under normal circumstances.

7. Æthyl-alcohol quickly stops the amœboid movements of the colourless corpuscles of the blood, and when sufficiently strong dissolves both them and the red corpuscles.

8. The addition of æthyl-alcohol to blood withdrawn from a vein retards putrefaction and prevents the development of the lower organisms. Arterial blood, however, taken from an intoxicated animal, putrefies more quickly than ordinary blood.

9. The action of æthyl-alcohol upon the heart is that it accelerates it, the acceleration being followed by a retardation. On pushing the alcohol farther, the pulse again becomes more frequent. These phenomena depend on the one hand upon excitation of the accelerating cardiac nerves, and on the other hand upon the exalted and subsequently enfeebled excitability of the inhibitory fibres of the vagi. In general the excitability of the accelerating cardiac nerves withstands the paralysing influence of alcohol for a longer period than the vagi. Æthyl-alcohol exerts some influence also upon the muscular tissue of the heart itself, which under certain circumstances is indicated by a prolongation and increased energy of the cardiac beat.

10. The pressure of the blood in the arteries is at first increased by æthyl-alcohol, but is subsequently depressed. The excitability of the vaso-motor centre is parallel to the blood-pressure. If the blood-pressure has already fallen, neither asphyxia nor the reflectorial excitation of the vaso-motor centre through the centric extremity of the vagus nerves induces the usually observed phenomena.

11. The rapidity of the blood-current in the carotid artery in an animal under the influence of alcohol is first increased, but diminishes as alcohol narcosis supervenes.

12. In regard to the respiration, when small quantities of æthyl-alcohol are ingested into the stomach, or injected into the veins, it is more rapidly performed, but with large doses it is rendered slower. The depth and rhythm of the respiration are also altered. When the respiration is rendered slower it is also less deep. The inspirations are slower. Reflex excitation through

the vagus upon the respiratory centre, which usually accelerates the respiratory movements, acts more feebly in proportion as the animal becomes more thoroughly intoxicated. If alcohol be injected into the jugular vein, and the vagi be intact, a prolongation of the acts of respiration immediately takes place; but if the vagi have been previously divided, acceleration occurs. The change in the frequency of the acts of respiration under the influence of alcohol, depends partly upon the action of the latter on the medulla oblongata, and partly on its action on the termination of the sensory fibres of the vagus in the lungs.

13. The temperature of the body generally falls during the action of alcohol.

14. The reflex action that can be excited through the skin of frogs that have been beheaded, rises in the first instance when the animal is under the influence of alcohol and subsequently falls.

15. The excitability of the motor and sensory nerves first rises and then falls.

16. The force of muscular contraction behaves in the same manner.

17. The quantity of lymph in the ductus thoracicus is at first augmented, but is subsequently diminished.

18. The same is observable in regard to the secretion of the gastric juice.

19. The quantity of urine increases though the peristaltic movements of the ureter occur more rarely.

20. The secretion of saliva from the submaxillary gland is increased whether the alcohol is injected into the stomach or directly into the blood.

21. The action of æthyl-alcohol upon the nervous system is direct, and is not occasioned by any changes in the blood or in the circulation.

22. Methyl, propyl, butyl, and amyl alcohols act in the same manner as æthyl-alcohol, but these several alcohols differ between themselves in the activity of the operation. This difference in the strength of the uniatomic saturated alcohols stands in intimate relation with the difference ( $\text{CH}_2$ ) of their composition. In correspondence with this, methyl alcohol acts more feebly than æthyl-alcohol and propyl, butyl, and amyl-alcohols in succession more and more strongly.

**Treatment of Spinal Congestion and Locomotor Ataxy.**—Dr. George Beard, of Brooklyn, Long Island, remarks that spinal congestion is a condition usually ignored or confounded with various other diseases, yet it is important that it should be recognised, since it comes on suddenly and is very amenable to treatment, whilst, if neglected, it may lead to

structural lesion. The disease is not characterised by any one symptom, but there are a large number which, taken collectively, establish the diagnosis. These, taken in the order of their relative importance, are—headache; stiffness of the neck, with tenderness and pain in the cervical vertebræ; heat and pain down the spine, with or without tenderness; feeling of pressure on the chest; dyspnoea and palpitation; feeling of constriction round the abdomen; disorders of the bladder and rectum; excessive sexual excitement, with erections and emissions; sensory and motor disturbances of a mild character in the arms and legs; jerking of the body or limbs; lastly, fixed paralysis of motion or sensation, or of both, in the limbs, with or without muscular contraction. Referring to the difficulties experienced in diagnosing between congestion and anæmia of the cord, Dr. Beard further indicates as points well worthy of attention in assisting the judgment—the history of the case; the fact that symptoms arising from spinal congestion are more or less fixed and permanent, and are usually worse when the patient is in a recumbent position; that spinal *congestion* is more likely to occur in the sanguine, bilious, and lymphatic temperaments—spinal *exhaustion* in the nervous temperament; that the female sex is less disposed to congestion of the spine than the male sex; that the tenderness over the vertebrae is usually much greater when the cord is exhausted than when it is congested; and finally, the results of treatment should be taken into consideration.

The treatment of fixed and active spinal congestion consists in the proper use of electricity, especially of the galvanic current, in the form of central galvanisation if one chooses, and if other associated symptoms require it; in the form of galvanisation of the spine, and the free use of ergot or iodide of potassium. Faradisation with a pleasant and gentle current is also of value in spinal congestion, though usually it is inferior to galvanism. Both currents, faradaic and galvanic, have a most powerful contracting influence on involuntary muscles, contraction taking place at both poles as well as with both currents, and it consists in a steady and gradual drawing together of the muscular substance without any immediate relaxation; in this respect involuntary muscles, Dr. Beard has shown, act very differently from voluntary muscles, which, except when they are greatly fatigued, relax immediately after contraction. The positive pole, Dr. Beard has satisfied himself, has a stronger contracting influence on involuntary muscles than the negative pole. This differential action applies to both currents, and apparently without regard to the direction of the current. He thinks the influence of current direction in the treatment of this and of other affections of the cord has been overdrawn.



In regard to the treatment of ataxia, Dr. Beard observes that it may be treated electrically by a combination of several different modes of application—galvanisation of the spine, central galvanisation, and general faradisation; when cerebral disturbance or general ataxy of the nervous system appears, galvanisation of the cervical sympathetic and peripheral faradisation with sponges and the metallic brush.

The principles on which the electro-therapeutist treats ataxy are typical of the principles on which he will be likely to treat all forms of disease; they will of themselves suffice to indicate to what school he belongs. He who holds the half-truth that only the seat of disease should be treated will confine himself to electrification of the spine; he who cherishes the delusion that the faradaic current cannot affect the nerve-centres will use in this disease only the galvanic current; he who blindly adopts the wild generalisations about the differential action of the ascending and descending currents will prefer either one or the other direction, according to his pathological theories; he who interprets all local disease to be the result and expression of general disease will at once resort to general and central applications; he who suspects the sympathetic to be the subtle source of all human woe will furiously galvanise the cervical ganglia; he who knows nothing about pathology or theories, and cares nothing about them, will empirically treat the symptoms. The wise and well-cultured physician, with eyes open both to pathology and to experience, will try all methods, and in the light of the results will hold fast to those which in each case seem to do good. Dr. Beard found good results from simply treating the leading symptom—the anæsthesia—without any special reference to the cord. He does this by means of the metallic brush, or by a finely-pointed metallic electrode, making the application over the feet, legs, arms, and all parts of the body that are anæsthetic. He has found more good, in some cases, from this method than from galvanisation of the spine and all the other methods combined. When the anæsthesia is profound and permanent, currents of great strength are sometimes not only not disagreeable, but positively agreeable.

In recommending this method he does not recommend exclusive reliance upon it: it is to be used in alternation with the other methods of which he has spoken. It should not be forgotten that the reflex effect of powerful peripheral faradisation on the cord may be of greater service than galvanisation of the spine. (*Philadelphia Medical Times*, Nos. 117 and 118, 1874.)

**New Sudorific and Sialogogue.**—At the meeting of the Société de Biologie de Paris, April 11, 1874, M. le Dr. Coutinho, of Pernambuco, announced that he had obtained from Brazil a



plant the therapeutical effects of which were deserving of the attention of physiologists. The name applied to it is the Jaborandi, the infusion of the leaves of which in doses of about a drachm induces violent sweating, intense salivation, and an enormous amount of bronchial secretion. Experiments made with it at the Hôpital Beaujon by Prof. Gubler have demonstrated that its diaphoretic action is constant, and that it is an incomparable sialogogue. Its action is felt within a few minutes after it is taken, when an abundant perspiration covers the face and whole surface of the body. Saliva flows in such quantities that speech becomes almost impossible, and on several occasions a pint and a quarter or more has been collected in the course of two hours. It is a remarkable circumstance that the intervention of heat has comparatively little effect on the sudorific effects of jaborandi, which in this respect differs considerably from the ordinary diaphoretics. It is easy to see to what various uses this new drug may be applied. M. Coutinho has employed it at the commencement of affections produced by cold, in the course of eruptive fevers, in erysipelas, in bronchitis, and pneumonia. In the discussion which followed the reading of the paper, M. Rabuteau, who has himself experimented on the effects of jaborandi, fully confirmed the statements of the author of the paper. The leaves of this plant, which belongs to the natural order Rutaceæ, are oblong and lanceolate, and resemble those of the laurel. They smell like hay, and are tasteless. They do not appear to contain any alkaloid. (*Revue Scientifique* April 18, 1874.)

## Notes and Queries.

### CORRESPONDENCE.

BROMIDE OF SODIUM IN EPILEPSY.—We have received the following from Professor Binz :—

“Of the four points which Dr. Hollis objects to, the editor of this periodical has sufficiently explained the first one. As to 2 and 3, I have to say the following :—

“The epileptic patient took, besides bromide of potassium for some time, succus conii for nearly four months, and wore a seton for a still longer period. *The fits diminished somewhat in frequency.*<sup>1</sup> He then took Na Br, first 3, afterwards 15 grains, three times daily. Not a word is said in Dr. Hollis’ paper to indicate that the second seton was then removed. (I request the reader to look at page 83 in the August number of this journal for 1873, and to compare.) I therefore was led to suppose that the patient went on with it. The action of the conium on the whole course of the illness is likewise included in the above-quoted words of the author. The fits, *averaging in frequency three or four weekly*, were reduced by Dr. Hollis’ seton, &c., to *one occurring once a week upon the average*. At this period of decline of the epilepsy the use of Na Br began, and this altogether was the reason that from such evidence I could not accept Dr. Hollis’ conclusion that *NaBr was a powerful remedy in arresting or at all events diminishing attacks of epilepsy*.

“In Dr. Hollis’ fourth objection, we learn that the other patient was relieved of his frequent fits *immediately* after taking daily 15 grains of the Na Br; not, as I said, after taking 15 grains three times daily for some time).<sup>2</sup> In Dr. Hollis’ first paper no statement whatever is made as to that effect. This miraculous

<sup>1</sup> The italics are Dr. Hollis’ own words from both papers.

<sup>2</sup> “Long-continued time” is too strong a rendering of the original *längere zeit*.

success of such an innocent salt in such a small dose, in the hands of a scientific allopathist, was so striking to me that I regretted its being reported only in five lines; making the error of 15 grains instead of 5.

"I spoke, although erroneously, in favour of the reported observation, for Dr. Hollis in his reply acknowledges himself that greater doses have a better chance of doing good; and as to the time required, I think everyone knows how very improbable such *final cessation of epileptic fits immediately after the commencement of this* or any more energetic treatment really is.

"Dr. Hollis censures the incorrectness only of some absolutely unessential matters in my brief report. Speaking to the point, he tells us that in the meantime he has tried the NaBr upon upwards of a dozen fresh cases, and he still adheres to his previous opinion, *i.e.*, that NaBr. is a powerful remedy in arresting, or at all events diminishing, attacks of epilepsy (cf. *Practitioner*, August 1873, p. 83). It will not be difficult to test this experience in hospitals, where there are better means of detecting fallacies than in private practice. And if it turns out that such *immediate* effect in severe cases of epilepsy—*with four or five fits weekly at the average number*—is indeed the consequence of 5 grains of NaBr three times daily, I will gladly confess that I was mistaken, not in a secondary and very unimportant part of the question, but in the whole. Meanwhile Dr. Hollis must allow me to refer him to the concluding sentences of my treatise (*Practitioner*, January 1874, p. 18)."

## Department of Public Health.

### SANITARY LEGISLATION FOR IRELAND.

IRELAND is, or is about to be, in the throes of a Public Health Bill. It is an ill wind that blows nobody any good, and the wind that has wrecked our Public Health Bill of 1872 ought to blow some good to Ireland. It is fortunate for Ireland that a change of Government gives some chance that, warned by the fate of the late Government's administrative folly in sanitary matters, the present Administration may give heed to common sense and the views of persons who may be presumed, from their previous knowledge of the subject, to have formed their opinions upon public health legislation from a clear understanding of what such legislation should compass. Dr. Thomas Wrigley Grimshaw has dealt with the requirements of sanitary legislation in Ireland in a recent able pamphlet<sup>1</sup> which should command the earnest attention of all who have any responsible part in promoting such legislation. Dr. Grimshaw has treated his subject in the light of the recent *fiasco* of sanitary administration in England, and he has applied the lesson most instructively to the needs of Ireland.

The question of sanitary legislation in Ireland does not appear to present the same amount of difficulty as in England, by reason of the confusion and the number of different sorts of local authorities being less. But the difficulties arising from this kind of evil, although not so great in quantity as in this

<sup>1</sup> "Remarks on Impending Sanitary Legislation for Ireland." Dublin : Browne and Nolan.

country, are of the same gravity. Ireland, moreover, like England, suffers from a surfeit of sanitary laws, local and general. How closely the legislative sanitary requirements of the two countries run in the same grooves, is shown by the summary of requirements which Dr. Grimshaw gives for Ireland. These consist in a codification, consolidation, and amendment of the existing sanitary laws; uniform authorities without clashing of jurisdiction; convenient areas of administration, with easily workable sub-districts; a complete executive organisation; constant supervision by the Central Authority; security for a certain amount of independence for the local opinions of the Local Authorities; and the compulsoriness of all sanitary law except certain permissive powers granted to the Central Authority.

Dr. Grimshaw illustrates the necessity for codification from recent difficulties which the Public Health Committee of Dublin got into "in their abortive attempts to prevent the spread of the epidemic." Many of their efforts were frustrated by "the miserable state of the law;" and he adds: "Even those who most blame our sanitary authorities cannot help sympathising with persons vainly struggling in the legal mire of 'some fifty or sixty statutes.'"

In Ireland the Local Authorities in towns are Town Councils and Town Commissioners; in the country, Boards of Guardians and Grand Juries. The first problem for solution is to constitute uniform sanitary authorities from these different kinds of authorities. This problem may be solved, Dr. Grimshaw suggests, by "making it *incumbent* on each authority to delegate its sanitary powers to a Public Health Committee for the management of all sanitary matters in the district. The appointment of such a committee is now voluntary on the part of the Sanitary Authority, and would have remained so if the proposed Irish Public Health Bill of last year had become law. By making the appointment of such a committee compulsory, and giving power to the Local Government Board to make general rules for its constitution and the management of its business, a sufficient degree of uniformity," Dr. Grimshaw believes, "will be attained without the necessity of abolishing any of the present Local Authorities, or of creating any new



authority—either of which proceedings would lead to confusion and delay.” In rural districts Dr. Grimshaw would have the Public Health Committee composed half of elected and half of *ex-officio* guardians; in urban districts, partly of members of the Sanitary Authority, partly of members chosen from the ratepayers with the consent and approval of the Local Government Board.

With respect to convenient areas of administration sub-divided into easily workable sub-districts, Dr. Grimshaw first deals with the sub-districts. These, he thinks, should be the Dispensary districts, which are coincident with the Union Electoral Divisions. “The average area of each of these districts is fifty square miles, or about seven miles square, with an average population of about 8,000, living in about 1,500 houses.” These districts, however, are of irregular form, and it is desirable that this irregularity should be rectified if they be adopted for sanitary purposes. There is, moreover, a question of rating so closely connected with local sanitary administration, that Dr. Grimshaw is of opinion that “the passing of a Union Rating Act (to set aside the present system of electoral divisional rating) is almost a necessity for the efficient working of a new sanitary organisation, as it is also for smoothly working the present Poor-law Medical Service.” The number of dispensary districts in Ireland is 718.

The larger administrative areas would necessarily be of two kinds, the rural and the urban. Ireland for Poor-law purposes is divided into 163 unions, and for Registration purposes into 8 districts. Dr. Grimshaw suggests a plan, first proposed by Mr. Furlong and accepted by Dr. Maunsell, namely, that of embracing the unions in each of the eight registration districts (with the exceptions shortly to be named) into rural administrative areas, and appointing a chief Health Officer over each.

As to the urban administrative areas, Dr. Grimshaw would limit them to urban districts with populations over 10,000. Of districts of this magnitude there are only 14 in Ireland. Two of these (Dublin and Belfast) have populations over 100,000; one (Cork) has a population of nearly 100,000; five have populations of from 20,000 to 50,000; and six of from 10,000 to 20,000. Of the remaining districts, 106 in number, 31

have populations of from 5,000 to 10,000, and the rest consist of towns and villages with populations under 5,000. Dr. Grimshaw says: "In my opinion, it is only the class of towns having a population of above 10,000 inhabitants for which distinct urban districts should be constituted, and for which a special sanitary staff should be employed. The remaining towns should be included in the rural sanitary districts in which they may happen to be situated. The extent of each urban district is a matter of considerable importance and should be determined in each case by the Local Government Board, not, as in England, by the request of the authorities concerned, or even with their consent. The urban district should include the union in which the town is situated and all the suburbs of the town. The Dublin urban district should include both the Dublin unions and all the suburban townships. There are good reasons why this method should be adopted; it will leave room for the growth of the town, and avoid the necessity of frequent alteration of boundaries, and will besides avoid the clashing of jurisdictions and much facilitate the appointment of a Public Health Committee and officers."

The Executive Sanitary Organisation proposed by Dr. Grimshaw is as follows:—

1. A *Central Authority*, to consist of the Local Government Board. The Irish Board is formed of a President, who is the Chief Secretary for the time being; a Vice-President, Sir Alfred Power, K.C.B.; the Under Secretary for the time being; Dr. M'Donnell, the Medical Commis-sioner; and Mr. Bellew—five Commissioners in all. It has been proposed to abolish the Medical Commissionership, as it would appear that his duties are not solely of a professional character. If this be done, Dr. Grimshaw thinks that the present is not the time to make the change, and that it will be necessary to substitute for the Medical Commissioner a medical officer or adviser "similar to the office held by Mr. Simon in the English Local Government Office." We believe that the withdrawal of the Medical Commissioner from the Board at a time when the Board would have to undertake the complicated control of public health administration would probably prove as fatal to efficient work as the absence of Mr. Simon from the councils of the English Local

Government Board has proved fatal to the sanitary work of that Board. What guarantee would there be that the suggested Medical Officer of the Irish Board would not be shunted as the Medical Officer of the English Board has been shunted? Dr. Grimshaw says that the Medical Commissioner or adviser should be the *Chief Medical Officer* of the sanitary organisation. The Royal Sanitary Commission suggested the same position for Mr. Simon, and we see what has come of it under English Poor-law management.

2. The *Rural Medical Officers of Health*, it is suggested, should be Medical Inspectors appointed by the Local Government Board, and should be eight in number, supervising the eight districts already referred to as coincident with the eight registration districts, each being a combination of from 16 to 25 unions. "The Public Health Inspector should also be the Poor-law Medical Inspector, and his duties should run parallel with, but not interfere with, the non-medical inspector. It should be the duty of the Medical Inspector to receive the reports from all the Local Health Officers and take action upon the same; he should also supervise the general sanitary condition of his district, and, conjointly with the non-medical inspector, conduct such local inquiries as the Local Government Board may direct, into complaints against or disputes between local authorities. He should also exercise constant supervision over the local Health Officers, and give them advice and assistance in all difficult cases." Commenting upon certain objections more fanciful than real, Dr. Grimshaw pertinently observes: "The Medical Inspectors must exist, and in the interests of economy they should be utilised to the greatest possible extent."

3. *Urban Medical Officers of Health* should be appointed for districts constituted as before described. They "should not engage in private practice, but might with advantage be officers of public hospitals or professors in medical schools, thus keeping themselves *au courant* with the medical science of the day."

4. *Local Medical Officers of Health* to be the Dispensary medical officers, each officer acting for his own district. "The utilisation of the present Poor-law service will be the cheapest and most efficient mode of working a sanitary organization."

5. *Inspectors of Nuisances*, it is suggested, should be the Relieving Officers of the Union; and it is also suggested that the appointment of *Superintending Inspectors of Nuisances* would tend to efficiency.

The Chief Medical Officer and Medical Inspectors would be State officers; the Local Officer would be appointed by the Public Health Committee previously described, and should be paid half by the State. The higher posts in the Public Health service should be filled from the lower posts as the occasion occurs.

Such is briefly Dr. Grimshaw's scheme of executive organisation.

After commenting on the constant supervision to be maintained by the Central Authority, Dr. Grimshaw indicates the mode in which he thinks the necessary amount of independence could be secured for the Local Officers from local influence. This he thinks would best be obtained by the Public Health Committees appointing to both Health duties and Dispensary duties. Finally, Dr. Grimshaw offers some observations on the need of sanitary laws being compulsory, and adds that "three Acts of Parliament are required in order to fairly mobilise Irish sanitary machinery: (1) an Act to effect Sanitary Organisation; (2) an Act for the Amendment and Codification of Sanitary Law; and (3) an Act to substitute Union for Divisional Rating, and for the alteration of the mode of electing Local Urban and Rural Authorities."

## THE COLLEGE OF PHYSICIANS' MEMORIAL ON OVERCROWDING.

THE memorial which the College of Physicians has recently addressed to the Premier, on overcrowding of the poor in London, is a somewhat notable fact. A body like the College is naturally, we might say properly, somewhat Conservative; at any rate it would shrink, with propriety, from advocating extreme or Utopian views, more especially upon questions



which are not wholly medical. The case must have been strong, and the public will feel that it must, which induced a body composed mainly of the seniors of the medical profession, and of all kinds of different political opinions, to take the step of memorialising the Prime Minister on a social question, and soliciting a parliamentary interference with matters which can only be dealt with by the introduction of considerable municipal changes. It is not surprising that a considerable number of alarmists within the College urged that the memorial would be met either with contemptuous neglect or with a severe rebuff to its authors for meddling with matters which did not concern them. Happily, the result has been very different. The action of the College has been applauded by all the most important public journals, and the only fault found with the memorialists seemed to be, that they were content to point out the magnitude of the existing evil, and the impossibility of its being remedied by existing machinery, without prescribing the particular remedial legislation which in their opinion should be adopted.

It is obvious, however, that the President and Fellows of the College of Physicians could not with propriety have laid down any such defined scheme of legislation. They were only entitled to point out, with the peculiar force which their exceptional experience gives to their statement, the spreading nature of the evils connected with the insufficient and improper housing of the London poor. This they have done with decisive effect. They have directed attention, not one moment too soon, to the monstrous behaviour of railway companies, municipal bodies, &c., which, under the pretence of carrying out "improvements," have unhoused the poor by thousands, and thus aggravated indefinitely the already sufficiently serious problems connected with the decent housing of the labouring classes in convenient proximity to the scene of their daily work. They have proposed no wild scheme of general proscription of bad houses under pressure of an absolutist central authority. They leave it abundantly open, by their suggestions that the work of regenerating the dwellings of the London poor should be carried out in a natural and healthy manner by commercial enterprise; but they do touch upon one difficulty which no private enterprise



can possibly surmount at present, viz., the acquiring of proper and convenient sites. The *Saturday Review* has very properly remarked that legislation might be invoked, not only for this purpose, but also for preventing, far more thoroughly than can be at present done, the existence of improper and inadequate old buildings, which, by their cheapness, might fatally compete with the improved dwellings which the building societies would offer to the public.

Since the above was written the Charity Organisation Society has had an interview with Mr. Cross; and an exceedingly interesting debate in the Commons, on the motion of Mr. Kay-Shuttleworth, has elicited from the Home Secretary the gratifying promise that the Government will bring in a measure dealing with the whole question.

## THE INTERNATIONAL EXHIBITION: SANITARY APPLIANCES.—1. AUTOMATIC SEWAGE ARRANGEMENTS.

THE sub-section of the International Exhibition devoted to "Sanitary Apparatus and Construction," contains numerous appliances of considerable interest. We propose to devote a series of articles to the more novel of these, first directing attention to the "Patent Self-acting Flush Tank, for flushing drains, disposing of house slops, and other sanitary purposes" (No. 5,968 in the catalogue exhibited by Mr. Rogers Field, C.E.) The principle of this apparatus (to follow closely the description of the patentee) consists in concentrating the flow of small quantities of sewage, or water, by collecting them for a time and then discharging the accumulated supply with a sudden flush by means of an automatic syphon. Insignificant supplies, which of themselves would create instead of removing deposit, may thus be rendered most effective for flushing, and house slops may be disposed of inoffensively where no regular system of sewage exists, in the manner hereafter explained.

The apparatus is formed by a cylindrical water-tight iron or

stone-ware tank. This tank has a trapped inlet, which also forms a movable cover to give access to the inside of the tank and a socket for a ventilating pipe. The outlet consists of a syphon, so arranged that no discharge takes place till the tank is completely filled with liquid, when the syphon is brought into action and the contents are immediately discharged. The inner end of the syphon is protected by a strainer, and the outer end enters a discharging trough which is made to turn round so that its mouth may be directed as required to connect the tank with the line of outlet pipes. This trough has a cover which can be removed to give access for cleaning.

When used for *flushing drains*, all that is required is to fix the tank in some convenient position between the supply and the drain to be flushed, and to connect the supply with the inlet and the drain with the outlet of the tank. There is no house in which there is not sufficient waste water for flushing by means of this apparatus. The sink, or scullery slops, are generally available as a supply, and the tank is specially adapted for them as it forms the most perfect kind of trap, breaking the connection between the drains and the house and intercepting the fat. Where the drains have only slight fall, advantage can be taken of the height of the sink by placing the top of the tank above the ground. The drippings from a water tap, or the rain water from a roof may also be used as a supply. A very small quantity will start the syphon when the tank is once full, but should it occasionally remain full for any time in consequence of insufficient supply, a jug of water thrown on the grating of the inlet will immediately set the syphon in action.

When used for the *disposal of house slops, where no regular system of sewerage exists*, the flush tank enables all house refuse to be removed inoffensively—the bed-room slops being thrown down the basin at the top of the tank outside the house—and thus, where earth or other dry closets are used for the excreta, this apparatus supplies a complete sanitary system of drainage. For this purpose the outlet from the tank must be connected with sub-irrigation drains laid in a garden or other small plot of available ground. These drains may consist of common two-inch agricultural drain pipes, laid some ten or twelve inches

below the surface, which will allow the sewage to flow out of the joints into the soil and feed the vegetation. The concentration of the flow of the sewage effected by the sudden discharge of the tanks, forces the liquid rapidly along the pipes and prevents their being choked. The liquid can thus be distributed over a sufficient area of ground to give full opportunity for the soil to purify it on the principle of "intermittent downward filtration." The arrangement described has been in successful action for nearly six years at some cottages in Essex. In this case the drains are laid under about seven perches of land for each cottage, and probably half this quantity would be sufficient to purify the sewage. The tanks are here cleared out every two or three months, and the drains are taken up and cleared once in every one or two years, which is readily done for a few shillings as they are so near the surface. No other attention is required.

The apparatus also forms an excellent *fat trap*, as it intercepts the fat much more effectively than ordinary traps, in consequence of the intermittent action of the discharge allowing the water to cool, so that the fat is completely separated, instead of being more or less carried on through the trap into the drains, as is usually the case.

The principle of the above described self-acting flush-tank has also been applied on a larger scale to the purpose of sewage utilisation by irrigation and intermittent filtration. Mr. Rogers Field has devised and patented, in conjunction with Mr. J. Bailey Denton, C.E., an "*Automatic Sewage Meter*." This meter has no place in the present International Exhibition, but a consideration of it cannot properly be excluded when Mr. Field's automatic flush-tank is described. The object and capabilities of the automatic sewage meter are best described in the language of the patentees:—

"The object of this invention is to regulate, automatically, the quantity of sewage or other liquid which may be applied to land, either for the purpose of utilisation or purification. One of the most prominent difficulties which presents itself in the utilisation of sewage is the very different quantity which is discharged from sewers at different times. In many towns and villages the flow will be diminished, at certain periods, to a mere dribble; while at other times there will be a copious discharge. Hence, as it is essential to economy in sewage farming that the crops should receive only that quantity of sewage which will produce

the most fruitful growth, and it is equally essential to success in intermittent filtration that the soil used to purify the sewage should receive only the quantity which it is capable of purifying, it becomes almost a necessity to have a means of regulating the quantity of sewage to be dealt with. The 'Automatic Sewage Meter' performs this service in the most simple and perfect manner, during the night as well as during the day, and without any supervision. The sewage is made to flow into a tank of such capacity, that when the liquid rises to a given level, the tank holds the precise quantity it is desired to deliver to a certain area of land at one time. This tank—called the Meter-tank—is provided with a syphon outlet, which is brought into action as soon as the liquid rises to the given level. When this is reached, the liquid flows out of the tank ('automatically') through the syphon, and continues flowing until the level of the liquid in the tank has fallen to the inlet mouth of the syphon, when air is drawn or admitted into the syphon, and the discharge ceases. The sewage flowing into the tank then commences to fill in again—slowly or quickly, according to the rate of influx,—and as soon as it is full, the automatic discharge will be repeated, and the liquid can be applied either to the same area of land, or to another, as desired. The coarser matters floating in the sewage are intercepted in a separate chamber, called the 'interception chamber,' before the meter-tank is reached—the finer particles being carried forward with the liquid through two strainers into the meter-tank. If it be desired to effect a precipitation of these finer particles, the space in the meter-tank below the inlet mouth of the syphon is increased in depth, so as to form a receptacle for any amount of deposit which it may be determined to precipitate and consolidate. In such case the space *above* the inlet mouth of the syphon will be occupied by classified liquid, to be discharged on the land automatically by the syphon, as already described, while the space *below* the said inlet will serve as a receptacle for the deposit, means being provided for draining off the liquid with which the precipitated particles are mixed, so as to allow of the consolidation of the deposit for removal. While this treatment of the solid portion is going on, the sewage is diverted into another meter-tank.

"The meter-tanks are multiplied in number according to the quantity of sewage under treatment and the use to which the precipitated matter is to be applied. Where the quantity of deposit is trifling, it can be flushed out from the meter-tank periodically without consolidation, and applied directly to the land. The Automatic Sewage Meter is applicable to towns of all sizes, but it will especially commend itself in small towns and villages, as well as in isolated country mansions, asylums, hospitals, schools, and other public buildings, where the floor of sewage is not only variable, but at times so small as to prevent its even distribution over land. No night labour is necessary. Before leaving the land, the man in charge of the sewage 'sets his stops' in the corners and furrows so as to secure the even distribution of the next discharge of sewage over the precise area to which it is intended it should be applied. The simplicity of the invention will be manifest from this description, and it will be seen that, in addition to regulating the quantity of sewage automatically and in the most perfect way, the syphon delivers the sewage in precisely the manner most suitable for irrigation or intermittent filtration—viz., at a gradually decreasing rate of discharge. Thus, at the commencement of the action of the syphon, when the carriers are empty, the land dry, and the absorption of sewage rapid, the discharge is rapid; and at the end of the action, when the carriers are full, the land wet, and the absorption of sewage comparatively slow, the discharge is slow."



We have had an opportunity of examining the operation of the self-acting flush tank at the cottages referred to in the description, also of the Automatic Sewage Meter at the hamlet of Eastwick, near Leatherhead. In both instances the tanks have been in operation for several years, and the objects for which they were designed seem to have been most efficiently obtained. The sewage meter at Eastwick has two chambers, each with its syphon, each having a capacity of 500 gallons, and each discharging itself in dry weather three times in two days. The sewage is distributed over somewhat less than an acre of ground. So far as a judgment may be formed from the example of the application of these inventions referred to, the "self-acting flush tank" and "automatic sewage meter" appear to be admirably fitted to prevent nuisance from and to utilise slops and sewage under the particular circumstances described by the inventors.

### THE EXCEPTIONAL TEMPERATURE OF THE PAST WINTER (1873-74).

[The following article, signed "H. Wild," and translated from the *Journal de St. Pétersbourg* of April 11, relates to a subject which has peculiar interest with reference to the state of health of this country during the past winter.]

THE past winter has been so exceptional, not only for St. Petersburg, but also for the greater part of Europe, that the public will no doubt be interested in receiving some precise data on the subject.

The extraordinary mildness of the winter in St. Petersburg has its scientific expression in the fact that the mean temperatures of the winter months, properly so called—that is to say of December, January, and February—were markedly above the average, that is to say above the mean temperatures observed during a long series of years. Thus, taking the mean of 90 years, the normal temperature of December in St. Peters-



burg is— $6^{\circ}8$  Centigrade ( $5^{\circ}4$  Réaumur), while the mean of December last was only  $-4^{\circ}5$  C.; the corresponding normal temperature of January is  $-9^{\circ}4$  C. ( $7^{\circ}5$  R.), but the mean of last January was  $-2^{\circ}6$  C.; finally, the normal temperature of February is  $-8^{\circ}2$  ( $6^{\circ}6$  R.), while the mean temperature of last February was but  $-6^{\circ}1$  C. In other words, December was on the average warmer by  $2^{\circ}3$  C. ( $1^{\circ}8$  R.), January by  $6^{\circ}8$  C. ( $5^{\circ}4$  R.), and February by  $2^{\circ}1$  C. ( $1^{\circ}7$  R.) Moreover, March has not yielded the compensation which was very generally expected, as the mean temperature of this month was  $0^{\circ}5$  C. above the normal temperature.

The data that to the present are accessible show that the unusually high temperature which existed in St. Petersburg during the winter months, existed also over all Europe as high as the 45th degree of latitude. The excess was most marked towards the north and north-east, whilst towards the south and south-east the amount gradually diminished. Thus, in Sweden and in Iceland the temperature has often been from  $10^{\circ}$  C. to  $20^{\circ}$  C. above the mean; in Central Europe the excess was much less; while in Southern Europe the temperature oscillated about the mean or fell below it.

Accepting the opinion very generally entertained at the present day, and not without reason, that in the temperature of the northern hemisphere a compensatory variation is observed—that is to say, in other words, that when there is an excess of warmth in some regions there is also a proportionate excess of cold in other regions—we cannot discover this compensation for the excessive temperature of Central and Northern Europe in the past winter in the South of Europe, where the temperature was either normal or but little below the average, and we must seek for the compensation beyond the limits of Europe. Let us look first towards Asiatic Russia, and inquire whether the winter has been exceptionally cold in Siberia. Unfortunately, the number of meteorological stations in Siberia is still very limited, and as yet the results of their observations are but partially known. But so much as is known of them appears to show that in Western Siberia, as in Europe, the mean temperature was in excess of the average, while in Eastern Siberia the mean temperature was but a very little below the average.

Asiatic Russia, it may then be concluded, has not afforded the compensation of temperature for which we seek.

Notwithstanding the intervening ocean, we have received from the United States more complete data respecting the past winter than from Russia, thanks to the noble system of "Telegraphic bulletins of temperature" that the War Department of the United States inaugurated some years ago. According to this system, all the telegraphic lines are placed three times daily, for twenty minutes, at the disposal of the Signal Officers of the War Department for the purpose of exchanging meteorological bulletins between the eighty-nine stations among which the officers are distributed. These observations are not merely brought to one focus at Washington and there digested and published; they are also made public in a multitude of localities at the time, through the agency of the journals, so that everyone may become cognisant of the atmospheric conditions of the country in its whole extent. The chief of the Signal Officers, General Albert J. Myer, has the goodness to transmit to St. Petersburg also without delay all these publications, so that we know even at the present time the temperature of the United States to the close of February. Now, it results from these data that during the three months of winter the temperature of the United States has been everywhere in excess. In December the excess above the average was  $4^{\circ}4$  C. ( $3^{\circ}5$  R.), in January  $3^{\circ}4$  C. ( $2^{\circ}7$  R.), in February  $1^{\circ}5$  C. ( $1^{\circ}2$  R.)

Thus far, then, our researches have not led to the discovery of any compensation of temperature in the northern hemisphere for the unusually high temperature of the winter in Europe, and we must search further, that is to say, either out at sea, or to the south of our previous observations, or even in the Arctic regions.

It is improbable that the atmosphere should have been unusually cold either above the Atlantic or above the Pacific, as there has been no indication of this being the case on the coasts of those oceans, and navigators have brought no news of such a fact. To the south of our observations the temperature is generally so regular below the 36th degree of latitude, that it would be bootless to seek for unusual cold there, and we are thus practically limited in our search to the polar regions.

To exceptional cold in the polar zone during this past winter the unusual warmth of the winter in Europe and in America is in truth to be attributed.

It is generally known that the variable temperature of the temperate zone is due to the alternation and irregular movement of the two principal atmospheric currents—the *equatorial current*, which passes from the south to the north, or from the south-west to the north-east, or, again, from west to east; and the *polar current*, which passes in an inverse direction. The equatorial current, coming from the south and from the ocean, brings to Europe air warm and humid, elevating the temperature (at least in winter), and having for accompaniments rain, snow, &c.; whilst the polar current, coming from the north, brings air cold and dry, and has for accompaniment (at least in winter) cold and clear weather. It is true that the most recent works have somewhat modified and complicated these elementary notions of atmospheric currents; and we have now to substitute for the equatorial current, cyclones or whirlwinds, in which the air (if viewed from above) would be seen to turn round a centre in the reverse direction of the hands of a watch, the centre itself moving in the directions indicated for the equatorial current. In the same manner the polar current is a species of whirlwind of colossal proportions, the movements of which, both with respect to its centre and round its centre, are in the reverse directions of the equatorial current. For this reason the polar current is termed the “anti-cyclone.” The two cyclones give rise to winds blowing in all possible directions, but while, for example, a wind coming from the north-west which forms part of a cyclone is in general cold and comparatively dry, a wind from the north or north-east, if it forms part of an anti-cyclone, will be humid and less cold, because it comes from the south and south-west.

The birthplace of cyclones is upon the ocean, in the most southerly latitudes and in the neighbourhood of the east coasts of the continents. Hence they travel from the south-west to the north-east towards the west coasts of the continents, and either extend to the north toward the Polar Sea, or move to the east over the continents. In ordinary years these two lines of propagation alternate, and if it be the first which predominates,

namely, the cyclone proceeding towards the Polar Sea, the continents are exposed more to the anti-cyclones which come from the glacial sea, and in that case we have rigorous winters. On the contrary, if the cyclones travel over the continents, they bring from the sea, in winter, a warmer air, and determine an elevation of temperature from the condensation of watery vapour and the formation of clouds.

These are the circumstances under which the warm winter of 1873-74 has taken place. It results from the synoptical charts prepared in the Central Physical Observatory for the Russian Empire, that last winter an extraordinary number of great cyclones travelled from the Atlantic by way of Scandinavia to the northern and central portions of Russia, and spread over into Eastern Siberia. From this cause it has arisen that the air in the north of Europe was much warmed along the track of the centres of the cyclones, whilst in Central Europe, which was but slightly affected by the cyclones, the temperature was less markedly raised. The same phenomenon was observed in the United States of North America, which were traversed by twelve cyclones in December and by eight in each of the two months of January and February.

When tempests rage principally on the continents in place of ascending to the glacial sea, the polar zone must doubtless undergo extraordinary cooling. In winter, when the arctic regions are deprived of the action of the sun, cyclones are the sole source of heat, and, indeed, a direct source, since they carry warm air from southern latitudes; and the watery vapour brought by them being condensed, releases the latent heat contained in the vapours. They are, moreover, an indirect source of heat, by reason of their covering the sky with clouds, and thus preventing the cooling of the air by radiation towards the colder sea. Now, as the greatest portion of the polar regions has been deprived of cyclones this winter, it is most probable that the zone of the North Pole, where the air remains stagnant, will have undergone an extraordinary cooling, and that, in consequence, the expeditions to the North Pole will meet this summer with great difficulties.

At the close of February there were indications of a great oceanic cyclone on the west coast of Europe, but the cyclone did



not traverse the Continent, but travelled towards the glacial sea. Under its immediate action, there arose near Archangel on the 26th and 27th Feb. an anti-cyclone, which, charged with heavy, cold air, proceeded the following days insensibly towards the south and south-east, producing disastrous cold in South Russia, then in Turkey, and in the south-east of Europe. On the northern coast of the Black Sea, in the Crimea, and in the Caucasus, the temperature fell at certain places twenty degrees below the average, and in Constantinople it remained many consecutive days ten degrees below.

On the 28th February, the south-east border of this anti-cyclone was felt at Novo-Rossysk as a violent *bora*, and it occasioned great tempests in the Black Sea. At the beginning of March the anti-cyclone invaded Central Europe, and the 3rd and 4th of the same month it had advanced towards the south-west of Europe, so that its posterior face, cold and humid, was observed at Saratov, at Stavropol, at Baku, at Novo-Rossysk, and at Soukhoun, under the form of a violent tempest of snow. At Constantinople snow commenced also to fall on the 3rd March, with a strong wind from the north-east, and continued to fall until the 7th March, whilst the wind, without diminishing in intensity, veered from north-east to the north and the north-west, conformably with the march of the anti-cyclone.

Thus this extraordinary snowstorm at Constantinople, of which the journals write as an altogether exceptional event, and which caused great damage, is to be regarded as the consequence of the anti-cyclone which carried the watery vapour from the Black Sea, and condensed it. Unfortunately, no observations are forthcoming from places south of Constantinople at the time of the snowstorm, and it is impossible to give more precise details of the area of the storm and of the circumstances which accompanied it.



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